ones it lasts several days; and this may possibly, combined with other observations, lead to a discovery of the real use of the corolla of plants, and the share it has in the impregnation, about which there has yet been no probable conjecture.

XIII. An Account of Experiments made by Mr. John M'Nab, at Albany Fort, Hudson's Bay, relative to the Freezing of Nitrous and Vitriolic Acids. By Henry Cavendish, Esq. F. R. S., and A. S. p. 166.

From the experiments made by Mr. M'Nab, of which I gave an account in the 76th volume of the Philos. Trans. p. 241, it appeared that spirit of nitre was subject, not only to what I call the aqueous congelation, namely, that in which it is chiefly, and perhaps entirely, the watery part which freezes, but also to another kind, in which the acid itself freezes, and which I call the spirituous congelation. When its strength is such as not to dissolve so much as $\frac{243}{1000}$ of its weight of marble, or when its strength is less than .243, as I call it for shortness, it is liable to the aqueous congelation solely; and it is only in greater strengths that the spirituous congelation can take place. This seems to be performed with the least degree of cold when the strength is .411, in which case the freezing point is at — 1½°. When the acid is either stronger or weaker, it requires a greater degree of cold; and in both cases the frozen part seems to approach nearer to the strength of .411 than the unfrozen part. The freezing points answering to different degrees of strength, seemed to be as annexed.

Strength.	Freezing points.	
.54	−31½°	spirit. congel.
.411	− 1½	
.38	−45¼	
.243	−44½	aqu. congel.
.21	−17	

As some of these properties however were deduced from reasoning not sufficiently easy to strike the generality of readers with much conviction, Mr. M'Nab was desired to try some more experiments to ascertain the truth of it; which he has executed with the same care and accuracy as the former. For this purpose, I sent him some bottles of spirit of nitre of different strengths, and he was desired to expose each of these liquors to the cold till they froze; then to try their temperature by a thermometer; afterwards to keep them in a warm room till the ice was almost melted, and then again expose them to the cold, and when a considerable part of the acid had frozen to try the temperature a 2d time; then to decant the unfrozen part into another bottle, and send both parts back to England, that their strength might be examined. The intent of this 2d exposure to the cold was as follows: spirit of nitre bears, like other liquors, to be cooled greatly below its freezing point without freezing: then the congelation begins suddenly; the liquor is filled with fine spicula of frozen matter, and the ice becomes so loose and porous, that if the process be continued long enough for a considerable portion of the acid to congeal, scarcely any of the fluid part can be

decanted: whereas, if it be heated in this state till the frozen part is almost, but not entirely, melted, and be again exposed to the cold, as the liquor is then in contact with the congealed matter, it begins to freeze as soon as it arrives at the freezing point, and the ice becomes much more solid and compact.

The intent of decanting the fluid part, and sending both parts back, that their strength might be determined, was partly to examine the truth of the supposition laid down in my former paper, that the strength of the frozen part approaches nearer to .411 than that of the unfrozen; but it is also a necessary step towards determining the freezing point answering to a given strength of the acid; for as the frozen part is commonly of a different strength from the unfrozen, the strength of the fluid part, and the cold necessary to make it freeze, is continually altering during the progress of the congelation. In consequence of this, the temperature of the liquor is not that with which the frozen part congealed; but it is that necessary to make the remainder, or the fluid part, begin to freeze, or in other words it is the freezing point of the fluid part. This is the reason that a thermometer, placed in spirit of nitre, continually sinks during the progress of congelation; which is contrary to what is observed in pure water, and other fluids in which no separation of parts is produced by freezing.

Further, from the above-mentioned experiments of Mr. M'Nab it appeared, that oil of vitriol, as well as spirit of nitre, is subject to the spirituous congelation; but it seemed uncertain whether, like the latter, it had any point of easiest freezing, or whether it did not uniformly freeze with less cold as the strength increased. For this reason, some bottles of oil of vitriol, of different strengths, were sent, which he was desired to try in the same manner as the former. This point indeed has since been determined by Mr. Keir, who has shown that oil of vitriol has strength of easiest freezing; and that at that point a remarkably slight degree of cold is sufficient for its congelation. The result of Mr. M'Nab's experiments on the nitrous acid is given in the following table.

N°	Decanted part.		Undecanted part.		Strength of the whole mass.	Strength before sent.	Freezing point by first method.	Freezing point by second method.
	Quantity.	Strength.	Quantity.	Strength.				
6	— —	— —	— —	— —	— —	.561	−41.6	— —
7	1410	.445	2137	.435	.439	.437	+ 1.7	− 3.8
8	1658	.390	1940	.422	.407	.408	− 3.5	− 4
9	1368	.353	2438	.416	.393	.391	− 4.5	−11
10	2206	.343	1920	.373	.357	.357	−12.5	−13.8
11	3620	.310	602	.381	.320	.320	−22.5	−23
12	2155	.276	1494	.293	.283	.280	−39.1	−40.3
13	1618	.241	1961	.235	.238	.238	−34	−32

The first column contains the numbers by which Mr. M'Nab has distinguished the different bottles. The 2d and 3d columns contain the quantity and strength

of the decanted part of the liquor; and the 4th and 5th show the quantity and strength of the undecanted part of the liquor. The 6th column gives the strength of both parts put together, or the strength of the whole mass; and the 7th is the strength of the same acid, as it was determined before it was sent to Hudson's Bay. The strengths of the decanted and undecanted parts were found by saturating the liquor returned home with marble; and that of the whole mass was inferred by computation from the quantity and strength of the decanted and undecanted parts; and as the strength thus inferred never differs from that determined before the liquors were sent to Hudson's Bay by more than $\frac{1}{100}$ part of the whole, it is not likely that the strengths of the decanted and undecanted parts here set down should differ from the truth by much more than that quantity. The 8th column contains the freezing points found in the first method, or the temperature of the liquors after the hasty congelation which took place on exposing them to the cold without any frozen matter in them; and the 9th contains their temperature after the more gradual congelation which took place when they were cooled with some frozen matter in them; and as the unfrozen part of the acid was decanted immediately after the temperature had been observed, it follows, that this column shows the true freezing points of the decanted liquors. In like manner the 8th column shows the freezing points of that part of the liquor which remained fluid in the first manner of trying the experiment; but as the strength of this part was not determined, the precise strengths to which these freezing points correspond are unknown. Thus much however is certain, that these points must be below those of the whole mass, and in all probability must be above those of the decanted liquor; as there is great reason to think that the quantity of frozen matter was always less, and consequently the strength of the fluid part differed less from that of the whole mass, in the first way of trying the experiment than in the second.

In all the foregoing acids the ice was heavier than the fluid part, and in consequence subsided to the bottom; a proof that it was the spirituous congelation which had taken place in them: but in N° 13 the frozen part swam at top, which shows that the congelation was of the aqueous kind.

As the temperatures in the 9th column of the foregoing table, are the freezing points answering to the strengths expressed in the 3d column; and as — 41¼ is the freezing point answering to the strength of .561; whence the freezing points determined by these experiments, and their respective strengths, are as annexed:

Strength.	Freezing point.
.561	— 41.6°
.445	— 3.8
.390	— 4
.353	— 11
.343	— 13.8
.310	— 23
.276	— 40.3

By interpolation from these data, according to Newton's method*, it appears

* Princip. Math. Lib. 3, prop. 40, lem. 5.

that the strength at which the acid freezes with the least cold is .418, and that the freezing point answering to that strength is — $2\frac{4}{10}$°.

In order to show more readily the freezing point answering to any given strength, I have computed, by the same method, the annexed table, in which the strengths increase in arithmetical progression. It was before shown that the freezing points, found by the first method, ought to be below those of the whole mass, and must in all probability be above those of the decanted liquor. In order to see how this agrees with observation, I computed in the above-mentioned manner the freezing points answering to the strength of the whole mass, and compared them with the observed freezing points. The result is given in the following table.

Strength.	Freezing point.	Difference.
.568	— 45.5	+ 15.4
.538	— 30.1	+ 12
.508	— 18.1	+ 8.7
.478	— 9.4	+ 5.3
.448	— 4.1	+ 1.7
.418	— 2.4	— 1.8
.388	— 4.2	— 5.5
.358	— 9.7	— 8
.328	— 17.7	— 10
.298	— 27.7	

N°	Strength of the whole mass.	Strength of the decanted liquor.	Computed freezing point of the whole mass.	Observed freezing point.	
				In first method.	In second method.
7	.439	.445	— 3.2	+ 1.7	— 3.8
8	.407	.390	— 2.6	— 3.5	— 4.
9	.393	.353	— 3.7	— 4.5	—11.
10	.357	.343	—10.	—12.5	—13.8
11	.320	.310	—19.9	—22.5	—23.
12	.283	.276	—35.6	—39.1	—40.3

It may be observed, that the freezing point of N° 7, tried in the first way, is considerably above that corresponding to the strength of the whole mass; but as this experiment appears to be doubtful, and not unlikely to exceed the truth, we may safely reject it as erroneous. All the others, as might be expected, are lower than those corresponding to the strength of the whole mass, and above those observed in the 2d manner; and therefore serve to confirm the truth of the above determination of the freezing points of spirit of nitre; and also show, that in this acid the point of spirituous congelation is pretty regular, and does not depend much, if at all, on the rapidity with which the congelation is performed.

The point of aqueous congelation, however, seems liable to considerable irregularity; for N° 13, after having been exposed to the cold, froze on agitation, the congelation being of the aqueous kind, and the thermometer stood stationary therein at — 34°. The ice being then almost melted, it was again exposed to the cold, till a good deal was frozen; but yet its temperature was then no lower than — $32\frac{1}{4}$°, though the quantity of frozen matter must certainly have been much more than in the first trial. The fluid part being then decanted, and the frozen

part melted, both were again exposed to the cold. They both were made to congeal by agitation, and the temperature of the undecanted was then found to be $-35°$, and that of the decanted part $-37°$: so that it should seem as if the freezing point found by the hasty congelation was always lower than that found the other way, which may perhaps proceed from this cause; namely, that when sufficient time is allowed, the watery part will separate from the rest, and freeze in a degree of cold much less than what is required to produce that effect, when it is performed in a more rapid manner.

These experiments confirm the truth of the conclusions I drew from Mr. M'Nab's former experiments; for, first, there is a certain degree of strength at which spirit of nitre freezes with a less degree of cold than when it is either stronger or weaker; and when spirit of nitre, of a different strength from that, is made to congeal, the frozen part approaches nearer to the foregoing degree of strength than the unfrozen. Also this strength, as well as its corresponding freezing point, and the freezing point answering to the strength of .54, come out very nearly the same as I concluded from those experiments; for by the present experiments they come out .418, $-2\frac{4}{10}°$, and $-31°$, and by the former .411, $-1\frac{1}{2}°$, and $-31°$. But the freezing point answering to the strength of .38 is totally different from what I there supposed. This must have been owing to the strength of that acid having been very different from what I thought it; which is not improbable, as its strength was inferred only from the quantity of snow which was added to it in finding the degree of cold produced by its mixture with snow.

On the Vitriolic Acid.—An irregularity of a remarkable kind occurred in trying 2 of these acids; namely, when the undecanted part was melted and again made to congeal, its freezing point was found to be much less cold than that of the decanted part, and the difference was much greater than could be attributed to the difference of strength. This seems to have happened only in the strongest 2 acids, namely, N° 1 and 2, and in great measure confirms the supposition which I formed from Mr. M'Nab's former experiments, that the congealed part of oil of vitriol differs from the rest, not merely in strength, but also in some other respect, which I am not acquainted with. It should seem however that this property does not extend to weak oil of vitriol. Some smaller irregularities occurred in trying the vitriolic acid; the cause of which I believe was, that when this acid has been cooled below the freezing point, and begins to freeze, the congelation proceeds but slowly; so that a considerable time elapses before it rises to the true freezing point. Something of the same kind seems to take place in the nitrous acid also, though in a less degree; for the decanted liquors usually continued to freeze and deposit a small quantity of ice, for a few minutes after they were poured off, though their cold, at least in some instances, was found rather to

diminish during that time. It must be observed, that small spicula of ice always came over along with the decanted liquor; and to this in all probability the new-formed ice attached itself; for otherwise it is likely that no ice would have been produced.

The following table contains the strength of the acids as determined before they were sent to Hudson's Bay, and the quantity and strength of the decanted and undecanted parts when they arrived at London, and the strength of the whole mass as computed from thence. For the sake of uniformity, I have expressed their strengths, like those of the nitrous acid, by the quantity of marble necessary to saturate them, though I did not find their strength by actually trying how much marble they would dissolve; as that method is too uncertain, on account of the selenite formed in the operation, and which in good measure defends the marble from the action of the acid. The method I used was, to find the weight of the plumbum vitriolatum formed by the addition of sugar of lead, and thence to compute the strength, on the supposition that a quantity of oil of vitriol, sufficient to produce 100 parts of plumbum vitriolatum, will dissolve 33 of marble; as I found by experiment that so much oil of vitriol would saturate as much fixed alkali as a quantity of nitrous acid sufficient to dissolve 33 of marble. It may be observed that the quantity of alkali, necessary to saturate a given quantity of acid, can hardly be determined with much accuracy, for which reason the foregoing less direct method was adopted; especially as the precipitation of plumbum vitriolatum shows the proportional strengths, which is the thing principally wanted, with as great accuracy as any method I know.

N°	Strength before sent.	Decanted part.		Undecanted part.		Strength of whole mass.
		Quantity.	Strength.	Quantity.	Strength.	
1	.977	1375	.967	3460	.963	.964
2	.918	3915	.919	1876	.905	.914
3	.846	88	.777	4915	.850	.849
4	.758	389	.710	3795	.753	.755
				547	.803	

The undecanted part of N° 4 was divided into 2 parts; viz. the less and the more congealable part; and it is the latter whose quantity and strength is given in the last line. It is well known that oil of vitriol attracts moisture with great avidity; and some of these acids were much exposed to the air during the experiments made with them, and may therefore be supposed to have attracted so much moisture from the air, as might sensibly diminish their strength; and this seems actually to have been the case with some of them. But as the bottles were well stopped, and as, except in one acid which was the most exposed to the air, the strength of the whole mass comes out not much less than that determined before the liquors were sent to Hudson's Bay, I imagine their strength could not sensibly

2

alter during their voyage home; and consequently their strength, at the time the last observations were made with them, could not differ much from that here set down.

It would be tedious to give the experiments for determining their freezing points in detail; but from these experiments it should seem, that the freezing point of oil of vitriol, answering to different strengths, is nearly as annexed: hence we may conclude, that oil of vitriol has not only strength of easiest freezing, as Mr. Keir has shown; but that, at a strength superior to this, it has another point of contrary flexure, beyond which if the strength be increased, the cold necessary to freeze it again begins to diminish. The strength answering to this latter point of contrary flexure must probably be rather more than .918, as the decanted or unfrozen part of N° 2 seemed rather stronger than the undecanted part; and for a like reason the strength of easiest freezing is rather more than .846. Mr. Keir found that oil of vitriol froze, with the least degree of cold, when its specific gravity at 60° of heat was 1.780, and that the freezing point answering to that degree of strength was + 46°: which agrees pretty nearly with these experiments, as the strength of oil of vitriol of that specific gravity is .848, that is, nearly the same as that of N° 3.

Strength.	Freezing point.
.977	+ 1°
.918	− 26
.846	+ 42
.758	− 45

A Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council. p. 191.

The result of the whole is as follows:

1787.	Thermometer without. Greatest height.	Thermometer without. Least height.	Thermometer without. Mean height.	Thermometer within. Greatest height.	Thermometer within. Least height.	Thermometer within. Mean height.	Barometer. Greatest height.	Barometer. Least height.	Barometer. Mean height.	Rain. Inches.
	°	°	°	°	°	°	Inches.	Inches.	Inches.	
January ..	51	27	39	54	45	49½	30.64	29.52	30.08	0.360
February	53	31	42	56	50	53	30.36	28.67	29.51	1.041
March ..	56	34	45	58	51½	54¾	30.51	29.03	29.77	1.283
April	58	37	47½	58	53	55½	30.48	29.07	29.77	0.923
May	71	39	55	64	53	58½	30.31	29.33	29.82	1.137
June	77	46	61½	67	59	63	30.29	29.55	22.92	0.662
July	78	51	64½	71	63	67	30.38	29.37	29.87	3.246
August ..	83½	48	65¾	73	62	67½	30.40	29.25	29.82	1.171
September	68	46	57	64	61	62½	30.45	29.11	29.78	0.990
October ..	63	39	51	62	54	58	30 15	29.21	29.68	1.818
November	56	29	42½	61	48	54½	30.45	29.12	29.73	1.340
December	53	30	41½	59	46	52½	30.40	29.21	29.80	3.000
Whole year			51			58			29.80	16.971

Explanation of the Instruments.—The instruments with which the foregoing observations were made are the same that were used in former observations of this

kind, a full account of which was given by Henry Cavendish, Esq. in the 66th volume of the Philos. Trans.; but as they have been moved from the situations they had at that time, it may not be amiss to mention how they are placed now, in order the better to show what degree of accuracy may be expected from them. There being no one situation for a thermometer out of doors so good as could be wished, it became necessary to make use of 2 thermometers; each is placed out of a three pair of stairs window, one facing E.N.E. and the other W.S.W. and they stand about 2 or 3 inches from the wall, that they may be the more exposed to the air, and the less affected by the heat and cold of the house. As the sun shines on the eastern part of the building in the morning, the thermometer to the westward is made use of for the morning observation during that season of the year when the sun rises high enough to affect the other; for all other observations, that to the eastward is employed. Neither the building opposite, nor that on the south side of the thermometer to the east, are elevated above it in an angle of more than 13°; but the opposite building is not more than 25 feet distant. The thermometer to the westward will not be affected by any other building than one to the northward, which is elevated above it in an angle of 20°, and which is only 20 feet distant. The thermometer within doors is placed close to the barometer, the heights of which it is intended chiefly to correct; the windows of the room in which they are kept look to the westward, and in winter the room has constantly a fire in it. The vessel which receives the rain is fixed to a chimney at the top of the house, and rises 6 inches above the chimney; it is not screened from the rain by any building or chimneys, there being none higher than that to which it is fixed.

*XIV. On the Natural History of the Cuckoo. By Mr. Edward Jenner.** p. 219.

The first appearance of cuckoos in Gloucestershire, the part of England where these observations were made, is about the 17th of April. The song of the male, which is well known, soon proclaims its arrival. The song of the female, if the peculiar notes of which it is composed may be so called, is widely different, and has been so little attended to, that I believe few are acquainted with it. I know not how to convey a proper idea of it by a comparison with the notes of any other bird; but the cry of the dab-chick bears the nearest resemblance to it.

Unlike the generality of birds, cuckoos do not pair. When a female appears on the wing, she is often attended by 2 or 3 males, who seem to be earnestly contending for her favours. From the time of her appearance, till after the middle of summer, the nests of the birds selected to receive her egg are to be found in

* Now Dr. Jenner, the celebrated discoverer of vaccination.

great abundance; but, like the other migrating birds, she does not begin to lay till some weeks after her arrival. I never could procure an egg till after the middle of May, though probably an early-coming cuckoo may produce one sooner.*

The cuckoo makes choice of the nests of a great variety of small birds. I have known its egg entrusted to the care of the hedge-sparrow, the water-wagtail, the titlark, the yellow-hammer, the green linnet, and the whinchat. Among these it generally selects the 3 former; but shows a much greater partiality to the hedge-sparrow than to any of the rest: therefore, for the purpose of avoiding confusion, this bird only, in the following account, will be considered as the foster-parent of the cuckoo, except in instances which are particularly specified.

The hedge-sparrow commonly takes up 4 or 5 days in laying her eggs. During this time, generally after she has laid 1 or 2, the cuckoo contrives to deposit her egg among the rest, leaving the future care of it entirely to the hedge-sparrow. This intrusion often occasions some discomposure; for the old hedge-sparrow at intervals, while she is sitting, not unfrequently throws out some of her own eggs, and sometimes injures them in such a way that they become addle; so that it more frequently happens, that only 2 or 3 hedge-sparrow's eggs are hatched with the cuckoo's, than otherwise: but whether this be the case or not, she sits the same length of time as if no foreign egg had been introduced, the cuckoo's egg requiring no longer incubation than her own. However, I have never seen an instance where the hedge-sparrow has either thrown out or injured the egg of the cuckoo. When the hedge-sparrow has sat her usual time, and disengaged the young cuckoo and some of her own offspring from the shell,† her own young ones, and any of her eggs that remain unhatched, are soon turned out, the young cuckoo remaining possessor of the nest, and sole object of her future care. The young birds are not previously killed, nor are the eggs demolished; but all are left to perish together, either entangled about the bush which contains the nest, or lying on the ground under it.

The early fate of the young hedge-sparrows is a circumstance that has been noticed by others, but attributed to wrong causes. A variety of conjectures have been formed on it. Some have supposed the parent cuckoo the author of their destruction; while others, as erroneously, have pronounced them smothered by the disproportioned size of their fellow-nestling. Now the cuckoo's egg

* What is meant by an early-coming cuckoo, I shall more fully explain in a paper on the migration of birds; but it may be necessary to mention here, that migrating birds of the same species arrive and depart in succession. Cuckoos, for example, appear in greater numbers on the 2d than on the 1st week of their arrival, and they disappear in the same gradual manner.—Orig.

† The young cuckoo is commonly hatched first.—Orig.

being not much larger than the hedge-sparrow's, it necessarily follows, that at first there can be no great difference in the size of the birds just burst from the shell. Of the fallacy of the former assertion also I was some years ago convinced, by having found that many cuckoo's eggs were hatched in the nests of other birds after the old cuckoo had disappeared; and by seeing the same fate then attend the nestling sparrows as during the appearance of old cuckoos in this country. But, before proceeding to the facts relating to the death of the young sparrows, it will be proper to state some examples of the incubation of the egg, and the rearing of the young cuckoo; since even the well known fact, that this business is entrusted to the care of other birds, has been controverted by the Hon. Daines Barrington; and since, as it is a fact so much out of the ordinary course of nature, it may still probably be disbelieved by others.

Exam. 1. The titlark is frequently selected by the cuckoo to take charge of its young one; but as it is a bird less familiar than many others, its nest is not so often discovered. I have however had several cuckoo's eggs brought to me that were found in titlark's nests; and had one opportunity of seeing the young cuckoo in the nest of this bird: I saw the old birds feed it repeatedly, and, to satisfy myself that they were really titlarks, shot them both, and found them to be so.

Exam. 2. A cuckoo laid her egg in a water-wagtail's nest in the thatch of an old cottage. The wagtail sat her usual time, and then hatched all the eggs but one; which, with all the young ones, except the cuckoo was turned out of the nest. The young birds, consisting of 5, were found on a rafter that projected from under the thatch, and with them was the egg, not in the least injured. On examining the egg, I found the young wagtail it contained quite perfect, and just in such a state as birds are when ready to be disengaged from the shell. The cuckoo was reared by the wagtails till it was nearly capable of flying, when it was killed by an accident.

Exam. 3. A hedge-sparrow built her nest in a hawthorn bush in a timber-yard: after she had laid 2 eggs, a cuckoo dropped in a 3d. The sparrow continued laying, as if nothing had happened, 'till she had laid 5, her usual number, and then sat. On inspecting the nest, June 20, 1786, I found that the bird had hatched this morning, and that every thing but the young cuckoo was thrown out. Under the nest I found 1 of the young hedge-sparrows dead, and 1 egg by the side of the nest entangled with the coarse woody materials that formed its outside covering. On examining the egg, I found one end of the shell a little cracked, and could see that the sparrow it contained was yet alive. It was then restored to the nest, but in a few minutes was thrown out. The egg being again suspended by the outside of the nest, was saved a second time from breaking. To see what would happen if the cuckoo was removed, I took

out the cuckoo, and placed the egg containing the hedge-sparrow in the nest in its stead. The old birds during this time flew about the spot, showing signs of great anxiety; but when I withdrew they quickly came to the nest again. On looking into it in a quarter of an hour afterwards, I found the young one completely hatched, warm and lively. The hedge-sparrows were suffered to remain undisturbed with their new charge for 3 hours, during which time they paid every attention to it, when the cuckoo was again put into the nest. The old sparrows had been so much disturbed by these intrusions, that for some time they showed an unwillingness to come to it: however, at length they came, and on examining the nest again in a few minutes, I found the young sparrow was tumbled out. It was a 2d time restored, but again experienced the same fate. From these experiments, and supposing, from the feeble appearance of the young cuckoo, just disengaged from the shell, that it was utterly incapable of displacing either the egg or the young sparrows, I was induced to believe that the old sparrows were the only agents in this seeming unnatural business; but I afterwards clearly perceived the cause of this strange phenomenon, by discovering the young cuckoo in the act of displacing its fellow-nestlings, as the following relations will fully evince.

June 18, 1787, I examined the nest of a hedge-sparrow, which then contained a cuckoo's and 3 hedge-sparrow's eggs. On inspecting it the day following I found the bird had hatched, but that the nest now contained only a young cuckoo and 1 hedge-sparrow. The nest was placed so near the extremity of a hedge, that I could distinctly see what was going forward in it; and to my astonishment, saw the young cuckoo, though so newly hatched, in the act of turning out the young hedge-sparrow. The mode of accomplishing this was very curious. The little animal, with the assistance of its rump and wings, contrived to get the bird on its back, and making a lodgement for the burden by elevating its elbows, clambered backward with it up the side of the nest till it reached the top, where resting for a moment, it threw off its load with a jirk, and quite disengaged it from the nest. It remained in this situation a short time, feeling about with the extremities of its wings, as if to be convinced whether the business was properly executed, and then dropped into the nest again. With these, the extremities of its wings, I have often seen it examine, as it were, an egg and nestling before it began its operations; and the nice sensibility which these parts appeared to possess seemed sufficiently to compensate the want of sight, which as yet it was destitute of. I afterwards put in an egg, and this, by a similar process, was conveyed to the edge of the nest, and thrown out. These experiments I have since repeated several times in different nests, and have always found the young cuckoo disposed to act in the same manner. In climbing up the nest, it sometimes drops its burden, and thus is foiled in its endea-

vours; but after a little respite, the work is resumed, and goes on almost incessantly till it is effected. It is wonderful to see the extraordinary exertions of the young cuckoo, when it is 2 or 3 days old, if a bird be put into the nest with it that is too weighty for it to lift out. In this state it seems ever restless and uneasy. But this disposition for turning out its companions begins to decline from the time it is 2 or 3 till it is about 12 days old, when, as far as I have hitherto seen, it ceases. Indeed, the disposition for throwing out the egg appears to cease a few days sooner; for I have frequently seen the young cuckoo, after it had been hatched 9 or 10 days, remove a nestling that had been placed in the nest with it, when it suffered an egg, put there at the same time, to remain unmolested. The singularity of its shape is well adapted to these purposes; for, different from other newly-hatched birds, its back from the scapulæ downwards is very broad, with a considerable depression in the middle. This depression seems formed by nature for the design of giving a more secure lodgement to the egg of the hedge-sparrow, or its young one, when the young cuckoo is employed in removing either of them from the nest. When it is about 12 days old, this cavity is quite filled up, and then the back assumes the shape of nestling birds in general.

Having found that the old hedge-sparrow commonly throws out some of her own eggs after her nest has received the cuckoo's, and not knowing how she might treat her young ones, if the young cuckoo was deprived of the power of dispossessing them of the nest, I made the following experiment. July 9. A young cuckoo, that had been hatched by a hedge-sparrow about 4 hours, was confined in the nest in such a manner that it could not possibly turn out the young hedge-sparrows which were hatched at the same time, though it was almost incessantly making attempts to effect it. The consequence was, the old birds fed the whole alike, and appeared in every respect to pay the same attention to their own young as to the young cuckoo, till the 13th, when the nest was unfortunately plundered.

The smallness of the cuckoo's egg, in proportion to the size of the bird, is a circumstance that hitherto I believe has escaped the notice of the ornithologist. So great is the disproportion, that it is in general smaller than that of the house-sparrow; whereas the difference in the size of the birds is nearly as 5 to 1. I have used the term in general, because eggs produced at different times by the same bird vary very much in size. I found a cuckoo's egg so light that it weighed only 43 grs., and one so heavy that it weighed 55 grs. The colour of the cuckoo's eggs is extremely variable. Some, both in ground and penciling, very much resemble the house-sparrow's; some are indistinctly covered with bran-coloured spots; and others are marked with lines of black, resembling in some measure the eggs of the yellow-hammer.

The circumstance of the young cuckoo's being destined by nature to throw out the young hedge-sparrows, seems to account for the parent-cuckoo's dropping her egg in the nests of birds so small as those I have particularized. If she were to do this in the nest of a bird which produced a large egg, and consequently a large nestling, the young cuckoo would probably find an insurmountable difficulty in solely possessing the nest, as its exertions would be unequal to the labour of turning out the young birds.* Besides, though many of the larger birds might have fed the nestling cuckoo very properly, had it been committed to their charge, yet they could not have suffered their own young to have been sacrificed, for the accommodation of the cuckoo, in such great number as the smaller ones, which are so much more abundant; for though it would be a vain attempt to calculate the numbers of nestlings destroyed by means of the cuckoo, yet the slightest observation would be sufficient to convince us that they must be very large. Here it may be remarked, that though nature permits the young cuckoo to make this great waste, yet the animals thus destroyed are not thrown away or rendered useless. At the season when this happens, great numbers of tender quadrupeds and reptiles are seeking provision; and if they find the callow nestlings which have fallen victims to the young cuckoo, they are furnished with food well adapted to their peculiar state.

It appears a little extraordinary, that 2 cuckoo's eggs should ever be deposited in the same nest, as the young one produced from one of them must inevitably perish; yet I have known 2 instances of this kind, one of which I shall relate. June 27, 1787, 2 cuckoos and a hedge-sparrow were hatched in the same nest this morning; one hedge-sparrow's egg remained unhatched. In a few hours after, a contest began between the cuckoos for the possession of the nest, which continued undetermined till the next afternoon; when one of them, which was somewhat superior in size, turned out the other, together with the young hedge-sparrow and the unhatched egg. This contest was very remarkable. The combatants alternately appeared to have the advantage, as each carried the other several times nearly to the top of the nest, and then sunk down again, oppressed by the weight of its burden; till at length, after various efforts, the strongest prevailed, and was afterwards brought up by the hedge-sparrows.

I come now, to consider the principal matter that has agitated the mind of the naturalist respecting the cuckoo; why, like other birds, it should not build a

* I have known an instance in which a hedge-sparrow sat on a cuckoo's egg and one of her own. Her own egg was hatched 5 days before the cuckoo's, when the young hedge-sparrow had gained such a superiority in size that the young cuckoo had not powers sufficient to lift it out of the nest till it was 2 days old, by which time it was grown very considerably. This egg was probably laid by the cuckoo several days after the hedge-sparrow had begun to sit; and even in this case it appears that its presence had created the disturbance before alluded to, as all the hedge-sparrow's eggs were gone except one.—Orig.

nest, incubate its eggs, and rear its own young? There is certainly no reason to be assigned, from the formation of this bird, why, in common with others, it should not perform all these several offices; for it is in every respect perfectly formed for collecting materials and building a nest. Neither its external shape nor internal structure prevent it from incubation; nor is it by any means incapacitated from bringing food to its young. It would be needless to enumerate the various opinions of authors on this subject, from Aristotle to the present time. Those of the ancients appear to be either visionary, or erroneous; and the attempts of the moderns towards its investigation have been confined within very narrow limits; for they have gone but little farther in their researches than to examine the constitution and structure of the bird, and having found it possessed of a capacious stomach with a thin external covering, concluded that the pressure on this part, in a sitting posture, prevented incubation. They have not considered that many of the birds which incubate have stomachs analogous to those of cuckoos: the stomach of the owl, for example, is proportionably capacious, and is almost as thinly covered with external integuments. Nor have they considered that the stomachs of nestlings are always much distended with food; and that this very part, during the whole time of their confinement to the nest, supports, in a great degree, the weight of the whole body; whereas, in a sitting bird, it is not nearly so much pressed on; for the breast in that case fills up chiefly the cavity of the nest, for which purpose, from its natural convexity, it is admirably well fitted.

These observations, I presume, may be sufficient to show that the cuckoo is not rendered incapable of sitting through a peculiarity either in the situation or formation of the stomach; yet, as a proof still more decisive, I shall state the following fact. In the summer of the year 1786, I saw, in the nest of a hedge-sparrow, a cuckoo, which, from its size and plumage, appeared to be nearly a fortnight old. On lifting it up in the nest, I observed 2 hedge-sparrow's eggs under it. At first I supposed them part of the number which had been sat on by the hedge-sparrow with the cuckoo's egg, and that they had become addle, as birds frequently suffer such eggs to remain in their nests with their young; but on breaking one of them I found it contained a living fœtus; so that of course these eggs must have been laid several days after the cuckoo was hatched, as the latter now completely filled up the nest, and was by this peculiar incident performing the part of a sitting-bird.*

Having under my inspection, in another hedge-sparrow's nest, a young cuckoo, about the same size as the former, I procured 2 wagtail's eggs which had been

* At this time I was unacquainted with the fact, that the young cuckoo turned out the eggs of the hedge-sparrow; but it is reasonable to conclude, that it had lost the disposition for doing this when these eggs were deposited in the nest.—Orig.

sat on a few days, and had them immediately conveyed to the spot, and placed under the cuckoo. On the 9th day after the eggs had been in this situation, the person appointed to superintend the nest, as it was some distance from the place of my residence, came to inform me, that the wagtails were hatched. On going to the place, and examining the nest, I found nothing in it but the cuckoo and the shells of the wagtail's eggs. The fact therefore of the birds being hatched, I do not give as coming immediately under my own eye; but the testimony of the person appointed to watch the nest was corroborated by that of another witness.

To what cause then may we attribute the singularities of the cuckoo? May they not be owing to the following circumstances? The short residence this bird is allowed to make in the country where it is destined to propagate its species, and the call that nature has on it, during that short residence, to produce a numerous progeny. The cuckoo's first appearance here is about the middle of April, commonly on the 17th. Its egg is not ready for incubation till some weeks after its arrival, seldom before the middle of May. A fortnight is taken up by the sitting bird in hatching the egg. The young bird generally continues 3 weeks in the nest before it flies, and the foster-parents feed it more than 5 weeks after this period; so that, if a cuckoo should be ready with an egg much sooner than the time pointed out, not a single nestling, even one of the earliest, would be fit to provide for itself before its parent would be instinctively directed to seek a new residence, and be thus compelled to abandon its young one; for old cuckoos take their final leave of this country the first week in July.

Had nature allowed the cuckoo to have staid here as long as some other migrating birds, which produce a single set of young ones, as the swift or nightingale for example, and had allowed her to have reared as large a number as any bird is capable of bringing up at one time, these might not have been sufficient to have answered her purpose; but by sending the cuckoo from one nest to another, she is reduced to the same state as the bird whose nest we daily rob of an egg, in which case the stimulus for incubation is suspended. Of this we have a familiar example in the common domestic fowl. That the cuckoo actually lays a great number of eggs, dissection seems to prove very decisively. On a comparison I had an opportunity of making between the ovarium, or racemus vitellorum, of a female cuckoo, killed just as she had begun to lay, and of a pullet killed in the same state, no essential difference appeared. The uterus of each contained an egg perfectly formed, and ready for exclusion; and the ovarium exhibited a large cluster of eggs gradually advanced from a very diminutive size, to the greatest the yolk acquires before it is received into the oviduct. The appearance of one killed on the 3d of July was very different: in this I could distinctly trace a great number of the membranes which had discharged yolks into the

oviduct; and one of them appeared as if it had parted with a yolk the preceding day. The ovarium still exhibited a cluster of enlarged eggs; but the most forward of them was scarcely larger than a mustard seed.

I would not be understood to advance that every egg which swells in the ovarium, at the approach or commencement of the propagating season, is brought to perfection; but it appears clearly that a bird, in obedience to the dictates of her own will, or to some hidden cause in the animal economy, can either retard or bring forward her eggs. Besides the example of the common fowl above alluded to, many others occur. If you destroy the nest of a blackbird, a robin, or almost any small bird, in the spring, when she has laid her usual number of eggs, it is well known to every one, who has paid any attention to inquiries of this kind, in how short a space of time she will produce a fresh set. Now had the bird been suffered to have proceeded without interruption in her natural course, the eggs would have been hatched, and the young ones brought to a state capable of providing for themselves, before she would have been induced to make another nest, and excited to produce another set of eggs from the ovarium. If the bird had been destroyed at the time she was sitting on her first laying of eggs, dissection would have shown the ovarium containing a great number in an enlarged state, and advancing in the usual progressive order. Hence it plainly appears, that birds can keep back or bring forward, under certain limitations, their eggs at any time during the season appointed for them to lay; but the cuckoo, not being subject to the common interruptions, goes on laying from the time she begins, till the eve of her departure from this country: for though old cuckoos in general take their leave the first week in July, and I never could see one after the 5th day of that month,* yet I have known an instance of an egg's being hatched in the nest of a hedge-sparrow so late as the 15th. And a further proof of their continuing to lay till the time of their leaving us may, I think, be fairly deduced from the appearances on dissection of the female cuckoo above mentioned, killed on the 3d of July.

Among the many peculiarities of the young cuckoo, there is one that shows itself very early. Long before it leaves the nest, it frequently, when irritated, assumes the manner of a bird of prey, looks ferocious, throws itself back, and pecks at any thing presented to it with great vehemence, often at the same time making a chuckling noise like a young hawk. Sometimes, when disturbed in a smaller degree, it makes a kind of hissing noise, accompanied with a heaving motion of the whole body.† The growth of the young cuckoo is uncommonly

* Though I am unacquainted with an instance, yet I conceive it possible, that here and there a straggling cuckoo may be seen after this time.—Orig.

† Young animals, being deprived of other modes of defence, are probably endowed with the powers of exciting fear in their common enemies. If you but slightly touch the young hedge-hog,

rapid. The chirp is plaintive, like that of the hedge-sparrow; but the sound is not acquired from the foster-parent, as it is the same whether it be reared by the hedge-sparrow, or any other bird. It never acquires the adult note during its stay in this country.

The stomachs of young cuckoos contain a great variety of food. On dissecting one that was brought up by wagtails, and fed by them at the time it was shot, though it was nearly of the size and fulness of plumage of the parent-bird, I found in its stomach the following substances. Flies and beetles of various kinds: small snails, with their shells unbroken: grashoppers: caterpillars: part of a horse-bean: a vegetable substance resembling bits of tough grass, rolled into a ball: the seeds of a vegetable that resembled those of the goose-grass.

In the stomach of one fed by hedge-sparrows, the contents were almost entirely vegetable; such as wheat, small vetches, &c. But this was the only instance of the kind I had ever seen, as these birds in general feed the young cuckoo with scarcely any thing but animal food. However, it served to clear up a point which before had somewhat puzzled me; for having found the cuckoo's egg in the nest of a green linnet, which begins very early to feed its young with vegetable food, I was apprehensive, till I saw this fact, that this bird would have been an unfit foster-parent for the young cuckoo. The titlark, I observe, feeds it principally with grashoppers. But the most singular substance, so often met with in the stomachs of young cuckoos, is a ball of hair curiously wound up. I have found it of various sizes, from that of a pea to that of a small nutmeg. It seems to be composed chiefly of horse-hairs, and from the resemblance it bears to the inside covering of the nest, I conceive the bird swallows it while a nestling. In the stomachs of old cuckoos I have often seen masses of hair; but these had evidently once formed a part of the hairy caterpillar, which the cuckoo often takes for its food.

There seems to be no precise time fixed for the departure of young cuckoos. I believe they go off in succession, probably as soon as they are capable of taking care of themselves; for though they stay here till they become nearly equal in size and growth of plumage to the old cuckoo, yet in this very state the fostering care of the hedge-sparrow is not withdrawn from them. I have frequently seen the young cuckoo of such a size that the hedge-sparrow has perched on its back, or half-expanded wing, in order to gain sufficient elevation to put the food into its mouth. At this advanced stage, I believe that young cuckoos procure some food for themselves; like the young rook for instance,

for instance, before it becomes fully armed with its prickly coat, the little animal jumps up with a sudden spring, and imitates very closely the sound of the word hush! as we pronounce it in a loud whisper. This disposition is apparent in many other animals.—Orig.

which in part feeds itself, and is partly fed by the old ones till the approach of the pairing season. If they did not go off in succession, it is probable we should see them in large numbers by the middle of August; for as they are to be found in great plenty,* when in a nestling state, they must now appear very numerous, since all of them must have quitted the nest before this time. But this is not the case; for they are not more numerous at any season than the parent birds are in the months of May and June.

The same instinctive impulse which directs the cuckoo to deposit her eggs in the nests of other birds, directs her young one to throw out the eggs and young of the owner of the nest. The scheme of nature would be incomplete without it; for it would be extremely difficult, if not impossible, for the little birds, destined to find succour for the cuckoo, to find it also for their own young ones, after a certain period; nor would there be room for the whole to inhabit the nest.

XV. On the Temperament of those Musical Instruments, in which the Tones, Keys, or Frets, are fixed; as in the Harpsichord, Organ, Guitar, &c. By Mr. T. Cavallo, F. R. S. p. 238.

The scale of music used at present consists of 7 principal notes or sounds, which musicians denote by the letters of the alphabet A, B, C, D, E, F, G; which, together with some intermediate ones, commonly called flats and sharps, and the octave of the first, make 13 sounds. When those sounds are considered with respect to the first, they are called by the following names, viz. the prime or key-note, the 2d minor, 2d, 3d minor, 3d major, 4th, 4th major, 5th, 6th minor, 6th major, 7th minor, 7th major, and octave.

Musical sounds are produced by the vibrations of the sonorous bodies, and they are acuter or graver as the vibrations performed in a given time are more or less in number; so that if a string vibrating 100 times in a second produces a certain sound, and another string vibrating 120 times in a second produces another sound, the latter is said to be acuter, higher, or sharper than the former. The number of vibrations performed in a certain time chiefly depends on the thickness, length, and elasticity of the sonorous bodies; but as the simplest sonorous bodies, and the fittest for examination, are those strings which are equal in every other respect, excepting in their lengths, because the number of vibrations, which they perform in a given time, is inversely in the proportion of their lengths, we shall consider only those in the present investigation, the number of vibrations performed by other sorts of sonorous bodies being easily deduced from them. As those 13 sounds are all different from each other, the strings which produce them differ in length, and of course in the number of

* I have known 4 young cuckoos in the nests of hedge-sparrows in a small paddock at the same time.—Orig.

the vibrations which they make in a certain time. Here follow the proportions which the times of vibration, or the length of the strings which express those 13 sounds, bear to the first, prime, or key note.

First.	1	Fourth.	$\frac{3}{4}$	Seventh minor.	$\frac{5}{9}$
Second minor	$\frac{15}{16}$	Fourth major	$\frac{32}{45}$	Seventh major	$\frac{8}{15}$
Second	$\frac{8}{9}$	Fifth	$\frac{2}{3}$	Octave	$\frac{1}{2}$
Third minor.	$\frac{5}{6}$	Sixth minor.	$\frac{5}{8}$		
Third major.	$\frac{4}{5}$	Sixth major.	$\frac{3}{5}$		

If, instead of many strings having those lengths in order to express the 13 sounds, or notes of an octave, one string be divided according to those proportions, and this string be stopped consecutively in the different points or divisions; on being struck, it will express the corresponding sounds. Thus, if a string stretched between 2 fixed points, be struck, it will produce a sound called the prime, first, or key-note; if it be stopped in the middle, one half of the string will sound the octave, its length, compared to that of the whole string, being in the proportion of 1 to 2; if $\frac{2}{3}$ of the string be caused to vibrate, the sound produced will be the 5th, its length, compared to that of the whole string, being as 2 to 3, and so of the rest. The highest sound of the octave is expressed by the half of the string; and if this half be divided again in the same manner or proportion, a higher octave will be obtained, the highest note of which will be expressed by a quarter of the original string. This quarter may be divided again into a higher octave, and so on; therefore, a string so divided may express the sounds of all the keys of a harpsichord or organ.

In regard to those divisions it may be observed, that as the notes of the 2d octave bear the same proportion to the first note of that octave as the notes of the first octave respectively bear to the first note of that octave, or to the whole string; and as the length of the string expressing the first note of the 2d octave, is half the length of the first note of the first octave, it follows, that the length of the string of every note in the 2d octave is half the length of the corresponding note in the first octave. Hence, when the divisions of the first octave are ascertained, in order to find the divisions of the notes of the 2d octave, we need only take the half of the lengths expressing the notes in the first octave. By the very same reasoning it is evident, that to find the divisions for the 3d octave, we need only take the halves of the lengths which express the notes of the 2d octave, or the quarters of those of the first octave, and so of the rest.

The first string or line is divided in the above-mentioned manner, and in order to avoid confusion, the divisions of the principal notes only of the first and 2d octave are annexed to it. Numbers are set under the line to express the lengths from the beginning to the divisions to which they stand near. The

letters just over the lines are the names of the notes or sounds expressed by the corresponding lengths of the string. The fractional numbers express the proportion which each particular division bears to the whole string; and the Roman numbers denote the numerical names of each note with respect to its distance from the first, which is always included. It is evident, that if any of those divisions be considered as the first or key-note, then the other notes, though they retain their alphabetical names, must have their numerical names altered accordingly: for example, if we take D for the key-note, then A will be the 5th of it, whereas A was the 6th when C was considered as the key-note; thus also B is the 3d of G, and the 7th of C; and so on.

Thus much having been premised, we may proceed to show the meaning of what is called the temperament in a system of musical sounds, and the necessity of it. For this purpose it is necessary to recollect, first, that the string, divided in the above-mentioned manner, exhibits the various notes or sounds of the keys of a harpsichord, the pipes of an organ, &c. 2dly, That those divisions remain unalterable, so that the harpsichord, when tuned, cannot be altered in the course of performing on it. And, 3dly, that when any of those notes or divisions is considered as the key-note, its 2d, 3d, 4th, 5th, &c. must bear their respective proportions, according to what has been said above. Now if, among the divisions of the first string CZ, we take D for the first or key-note, its length being 320 inches, the length of its 5th must be $213\frac{1}{3}$ inches, viz. $\frac{2}{3}$ of 320, that being the proportion which the 5th must bear to the key-note; but among the divisions of the string, there is none equal to $213\frac{1}{3}$ inches; therefore, there is not a note among them which may serve for a 5th to D; however, as the length of AZ, viz. 216, is the nearest to $213\frac{1}{3}$, this A must be taken for the 5th of D. It is evident, that this is an imperfect 5th of D; but if, in order to render it perfect, we make AZ equal to $213\frac{1}{3}$ inches instead of 216, then it will be a redundant 6th to C, when C is considered as the key-note; the best expedient therefore, is to divide the imperfection between the 2 lengths, viz. to make AZ neither so long as 216, nor so short as $213\frac{1}{3}$, which will render the disagreeable sensation, arising from the improper length, the least possible. This alteration of the just lengths of strings, necessary for adapting them to several key-notes, is called the temperament: and the best temperament in a set of musical sounds is evidently such a partition of the natural imperfections, as will render all the chords equally and the least disagreeable possible.

What has been exemplified in D and A may be said of all the other notes; so that if any one of them be a perfect 3d, 5th, &c. with respect to one key-note, it will be found to be imperfect with respect to others. Hence it is manifest, 1st, that in a set of musical keys, pipes, or frets, a temperament is absolutely necessary; and, 2dly, that the harpsichord, organ, guitar, or any other instrument

in which the notes are fixed, so as not to be alterable by the performer's hands, must be imperfect even when tuned in the best manner possible; for by the temperament we can divide, but not annihilate the imperfection. Other instruments, in which the notes are not fixed, as the violin, violoncello, &c. are perfect, because the performer stops the strings on them in different places, even for sounding the notes of the same name. Thus a skilful performer, in order to sound A, will stop the string a little farther from the bridge when he plays in the key of C, viz. when C is considered as the key-note, than when he plays in the key of D.

Most people imagine, that the scale of musick is capable of many different temperaments; and, agreeable to this supposition, the writers on harmonics have proposed different temperaments; but the nature of the scale admits of only one temperament capable of rendering the imperfection and the harmony equal throughout; and it is impossible to form a different and more advantageous scale. It may also be remarked, 1st, that the proportion of 2 to 3 for the fifth, the proportion of 1 to 2 for the octave, and in short the proportions of all the notes, are not assumed at pleasure; but have been determined from constant experience, viz. from the agreeable or disagreeable effects produced when 2 different notes are sounded at the same time.

Thus, let 2 strings equal in every respect be struck at the same time, and they will express the same sound precisely, so that no ear can perceive any difference between them, and it is almost impossible to distinguish whether the sound arises from 2 strings, or from 1 only, excepting from the loudness. But if one of those strings be successively stopped in different parts of its length, while the other remains open as before; and if at every time they be both struck together, their combined sounds will be found to produce different effects, viz. sometimes more or less pleasing, and at other times more or less disagreeable. When the combinations of the 2 sounds are agreeable, they are called concords; and when disagreeable, they are called discords. Experience evinces, that the best concord is when the length of one string is to the length of the other as 1 to 2, every other circumstance being the same in both. This proportion forms the octave. The next best concord is the 5th, viz. when the lengths of the two strings are as 2 to 3, after which come the proportions of 3 to 4, 4 to 5, 3 to 5, 5 to 6, and 5 to 8, for the other concords. The other proportions besides these are disagreeable in a greater or less degree, unless they are greater than the proportion of 1 to 2; but in that case it will be found, that the proportions which produce agreeable combinations are the double, quadruple, octuple, &c. of those mentioned above, viz. are their octaves, double octaves, &c.: thus the proportion of 1 to 4 produces a very agreeable concord, because 1 to 4 is the double of 1 to 2, viz. it expresses a double octave. 2dly, Hence if we have the length of a string, or the proportion of a note in any part of the string, we may easily find its

octaves by taking its double, or its half, or the double of the double, &c.: we may find its octave below by taking twice 90, viz. 180, or the octave of this octave, which is 360, viz. equal to twice 180, or to four times 90; and, on the other side, we may find the octave above of the given note by taking its half, which is 45, &c.

Mr. C. now shows why within the octave there are admitted only 13 different notes, viz. 8 principal ones, and 5 others, called sharps and flats. He assumes a line to represent a musical string, the length of which is supposed to be divided into a certain number of equal parts, suppose 13286025. On one side of this line are set the divisions of 7 successive octaves, viz. the half of it, a quarter of it, &c.; and on the other side are the divisions of a series of 5ths, viz. the 5th of the whole string, the 5th of this 5th and so on, which are found by taking $\frac{2}{3}$ of the whole string, then $\frac{2}{3}$ of those $\frac{2}{3}$, and so on. Here notice is taken only of the octaves and 5ths, because they are the principal and the best concords; so that a temperament being required, it is necessary first to take care that these concords be not rendered insufferable to the ear, the rest admitting of a greater latitude in the temperament or deviation from the perfect state. Besides, all the other notes are derived from the series of successive 5ths. In whatever key a piece of music is performed, its 5th is the most predominant of its concords; and as the notes of music must be so ordered as that, for the sake of modulation, any note may be considered as the key-note; therefore having found the 5th of the whole string by taking $\frac{2}{3}$ of its length, which gives a note called G, we must suppose, that this G may be considered as the key-note, consequently must find its 5th, which gives D, and so on, until we find one of those successive 5ths, which coincides with one of the successive octaves; for after that, to find more successive 5ths would be only repeating the same thing over again.

Indeed, if we carry the succession of octaves and of 5ths indefinitely far, we shall find, that no one of the 5ths ever coincides perfectly with one of the octaves, and therefore the division would have no end. However, as the length of the 7th octave comes so very near to the 12th fifth, we must be contented with taking this 7th octave for the 5th of F, the difference between them being about the 100th part of its length; whereas, if we carry on the succession of 5ths and of octaves, we shall find, that among 30 and more 5ths none comes nearer to one of the octaves than the above-mentioned one. Hence the number of 5ths in this series is 12; and as, when the division expressing a certain note has been assigned in any part of a string, we may easily find all its octaves above and below, it follows, that by finding all the octaves of those 12 divisions, we shall have 12 distinct notes within half the string, viz. within the first octave of the whole string; to which, if the sound of the whole string be added, we have 13

different sounds; which shows why an octave comprehends neither more nor less than 13 notes. Without dwelling any longer on the names or number of those notes, Mr. C. proceeds to find out the temperament.

It appears by the above divisions, that the length of the string for the last 5th is shorter than the length of the last octave, and also that one of them must necessarily be taken for both purposes; but here we must consult nature, examining by the ear which of the 2 is least disagreeable. This however is soon decided; for imperfect octaves are quite insufferable, whereas a certain degree of imperfection in the 5ths is tolerable; therefore we are necessitated to leave the octaves perfect, and to let the 7th octave serve for the 5th of F. In this case it is evident that each of the notes in the succession of 5ths is a perfect 5th to its preceding note, excepting the last, which would be by much too flat, and therefore it is necessary to divide the imperfection equally among them all. For this purpose it must be considered, that as the 12 successive 5ths, together with the whole string or first note, are each $\frac{2}{3}$ of its preceding note; they form a geometrical series, the ratio of which is $\frac{3}{2}$, its extremes are 13286025, the first length, and 102400, the 12th fifth, and the number of terms is 13. But because instead of 102400, which is the last 5th, we must take the number 103797, viz. the length nearly of the 7th octave, for the last term of the series; therefore the problem is reduced to the finding out of 11 mean proportionals between the two numbers 13286025 and 103797. Now by the nature of a geometrical progression, $\frac{13286025}{103797} = r^{12} = 128$, consequently $r = \sqrt[12]{128} = 1.4983$ the ratio of the series.

The ratio having been ascertained, the succession of tempered 5ths is thus easily determined; viz. divide the length of the whole string by this ratio, and the quotient gives the 1st tempered 5th; divide this 5th by the same ratio, and the quotient gives the 2d tempered 5th; divide this 2d 5th by the same ratio, and so on till the last 5th, which comes out equal to $103797\frac{6}{83}$, which is so nearly equal to the length of the 7th octave, that the difference is truly insignificant. The divisions, thus ascertained, form a series of notes, in which the octaves only are perfect; but all the 5ths, all the 3ds, and in short all the chords of the same denomination, are equally tempered throughout; so that whichever of them is taken for the key-note, its 5th, 6th, &c. will have always the same proportion to it, and consequently will always produce the same harmony when sounded with it. It is evident that, besides this, there can be no other temperament capable of producing equal harmony; for when the extremes of a geometrical series and number of mean proportionals are given, there can be only one set of those means. If, on the other hand, we endeavour to find a better temperament by introducing more than 13 notes within the limits of an octave, we shall find it

impracticable, because that after the number 13, if the succession of 5ths be carried on further, they will recede more from a coincidence with any one of the octaves.

This explanation of the nature, origin, and necessity of the temperament has been thought necessary for the sake of perspicuity; but the same end may be obtained by the following easier method. As the 13 notes of an octave must be arranged so, that whichever of them be taken for the 1st or key-note, the 2d, 3d, 4th, &c. may bear the same constant proportion to it; they must therefore be in a geometrical proportion, so as to form a series of 13 numbers, the extremes of which are the whole string and its half, viz. any number and its half. The ratio of this series is found in the same manner as in the other series, viz. the greatest extreme is divided by the least, and the 12th root of the quotient is the ratio sought. But the extremes are any assumed number and its half: and as the quotient of a number divided by the half of the same number is always equal to 2; therefore, whatever be the length of the string, the ratio is always $\sqrt[12]{2} = 1.0594+$; and if the length of the whole string be divided by this ratio, viz. $1.0594+$, the quotient will be the length of the string expressing the 2d note, which, divided by the same ratio, gives the 3d note, and so on; or else, instead of dividing the length of the whole string by the ratio, we may multiply the half of it by the ratio, the product of which will give the 7th note, which multiplied by the same ratio gives the 6th, and so on in a retrograde order, which will give the tempered notes of the octaves as well as the former method. By this means the annexed divisions for the notes of an octave have been calculated, the length of the whole string having been supposed equal to 100000.

I.	100000
* b	94387
II.	89090
* b	84090
III.	79370
IV.	74915
* b	70710
V.	66743
* b	62997
VI.	59462
* b	56123
VII.	52973
VIII.	50000

If a monochord be divided in this manner, and a harpsichord tuned by it, this instrument will then be tuned so, that whichever note be taken for the first or key-note, its 5th, 6th, &c. will produce the same effect respectively.

At present, the harpsichords and organs are commonly tuned so, that some concords are very agreeable to the ear, while others are quite intolerable; or, in other words, when the performer plays in certain keys, the harmony is very pleasing, in others the harmony is just tolerable, and in some other keys the harmony is quite disagreeable. The best keys to be played in, are the keys of C, of F, of E flat, of B flat, of G and of D in the major mood; and the keys of C, of D, of A, and of B, in the minor mood. Next to those come the less agreeable keys of A, A flat, and E in the major mood; besides those, the rest are disagreeable in a greater or less degree, so that out of 12 keys, which, on

account of the 2 moods, viz. the major and the minor, become 24, there are hardly 14 that can be used; and for this reason most of the modern compositions in musick are written in those keys.

So far the common method of tuning answers some purpose; for as long as the performer is to play in certain keys only, it is much better to have them tuned in the most advantageous manner, than to let those be tuned in a less perfect way for the sake of others, which he does not intend to use. Hence the great harpsichord players generally have their instruments tuned in a peculiar manner, viz. so as to give the most advantageous effect to those concords which they more frequently use in their compositions. And hence also the harpsichords and organs are always tuned different from each other, unless they be tuned by the same person with equal attention, and without any particular instructions. This practice cannot conveniently be laid aside, viz. when the instrument is to be tuned for solo playing; and for a certain style of music, it is very proper to tune it so as to give the greatest effect to these combinations of sounds which are mostly used in those compositions. But the case is far different when the instrument is to serve for accompanying other instruments in every sort of music, or the voices of good singers; for then the disagreement becomes very audible; and for this purpose the harpsichord or organ ought to be tuned according to the above demonstrated temperament of equal harmony, which is the only one that can possibly take place.

When the compositions of old masters are performed in concert, and with the organ or harpsichord tuned in the common manner, the effect is frequently very disagreeable. This is particularly the case with the songs of Handel, Galluppi, Leo, Pergolese, and others, who wrote in a great variety of keys, and very often in those for which the common way of tuning is not at all calculated.

XVI. Description of a New Electrical Instrument capable of Collecting together a Diffused or Little Condensed Quantity of Electricity. By Mr. T. Cavallo, F.R.S. p. 255.

One of the principal desiderata in practical electricity has been a method of ascertaining the presence and quality of suchdiffused or weak electricity as could not immediately affect an electrometer; of this nature is the electricity produced by effervescences and other processes, also the electricity of the atmosphere in serene and warm weather, &c. M. Volta's condenser, described in volume 72 of the Philos. Trans. was the first attempt of the kind, and indeed when this instrument is in good order it answers exceedingly well; but the difficulty of constructing and of preserving it, added to the frequent uncertainty of the result, have occasioned its being little, if at all, used by those who study the subject of electricity. Mr. Bennet's doubler, described in vol. 77 of the Philos. Trans. was

also intended to manifest small, and otherwise unperceivable, quantities of electricity; but from the experiments and observations Mr. C. since laid before the R. S. he thinks it clearly shown, that this doubler cannot be of any use, on account of its being naturally always electrified. In the same paper he mentioned a method which he had used for collecting diffused quantities of electricity. Since that time he has improved the method; and, after several alterations, constructed an instrument for the purpose, which is deemed free from all those faults which render M. Volta's and Mr. Bennet's instruments of little, if at all of any, use.

The properties of this machine, which from its office may be called a collector of electricity, are first, that when connected with the atmosphere, the rain, or in short with any body which produces electricity slowly, or which contains that power in a very rarefied manner, it collects the electricity, and afterwards renders both the presence and quality of it manifest, by communicating it to an electrometer. 2dly, This collecting power, by increasing the size of the instrument, and especially by using a 2d or smaller instrument of the like sort to collect the electricity from the former, may be augmented to any degree. 3dly, It is constructed, managed, and preserved with ease and certainty; and it never gives, nor can it give, he thinks, an equivocal result.

Fig. 1 and 2, pl. 6, exhibit 2 perspective views of this collector. Fig. 1 shows the instrument in the state of collecting the electricity; and fig. 2 shows it in the state in which the collected electricity is to be rendered manifest. An electrometer is annexed to each. The letters of reference indicate the same parts in both figures. ABCD is a flat tin plate, 13 inches long and 8 inches broad; to the 2 shorter sides of which are soldered 2 tin tubes AD and BC, which are open at both ends. DE and CF are 2 glass rods covered with sealing wax by means of heat, and not by dissolving the sealing wax in spirits. They are cemented into the lower apertures of the tin tubes, and also in the wooden bottom of the frame or machine at E and F, so that the tin plate ABCD is supported by those glass rods in a vertical position, and is exceedingly well insulated. GHILKM and NOPV are 2 frames of wood which, being fastened to the bottom boards by means of brass hinges, may be placed so as to stand in an upright position and parallel to the tin plate, as shown in fig. 1; or they may be opened, and laid on the table which supports the instrument, as in fig. 2. The inner surfaces of those frames from their middle upwards is covered with gilt paper XY; but it would be better to cover them with tin plates, hammered very flat. When the lateral frame stands straight up, they do not touch the tin plate; but they stand at about $\frac{1}{4}$ part of an inch asunder. They are also a little shorter than the tin plate, that they might not touch the tin tubes AD, BC. In the middle of the upper part of each lateral frame is a small flat piece of wood S and T, with a brass hook; the use of which is to hold up the frames without the danger of

their falling down when not required, and at the same time it prevents their coming nearer to the tin plate than the proper limit. It is evident that when the instrument stands as shown in fig. 1, the gilt surface of the paper XY, which covers the inside of the lateral frames, stands contiguous and parallel to the tin plate.

When the instrument is to be used, it must be placed on a table, a window, or other convenient support, a bottle electrometer is placed near it, and is connected, by means of a wire, with one of the tin tubes AD, BC; and by another conducting communication the tin plate must be connected with the electrified substance, the electricity of which is required to be collected on the plate ABCD: thus, for instance, if it be required to collect the electricity of the rain, or of the air, the instrument being placed near a window, a long wire must be put with one extremity into the aperture A or B of one of the tin tubes, and with the other extremity projecting out of the window. If it be required to collect the electricity produced by evaporation, a small tin pan, having a wire or foot of about 6 inches in length, must be put on one of the tin tubes, so that the wire going into the tube the pan may stand about 2 or 3 inches above the instrument. A lighted coal is then put into the pan, and a few drops of water poured on it will produce the desired effect.

Mr. C. adds, that having actually used this new instrument in several experiments, he had found it to answer perfectly well; one of its principal recommendations being the certainty of its operation.

XVII. On the Conversion of a Mixture of Dephlogisticated and Phlogisticated Air into Nitrous Acid, by the Electric Spark. By Henry Cavendish, Esq., F. R. S., and A. S. p. 261.

In volume 75 of the Phil. Trans., p. 372, I related an experiment, which showed, that by passing repeated electric sparks through a mixture of atmospheric and dephlogisticated air, confined in a bent glass tube by columns of soap-lees and quicksilver, the air was converted into nitrous acid, which united to the soap-lees and formed nitre. But as this experiment has since been tried by some persons of distinguished ability in such pursuits without success, I thought it right to take some measures to authenticate the truth of it. For this purpose, I requested Mr. Gilpin, clerk of the R. S., to repeat the experiment, and desired some of the gentlemen most conversant with these subjects to be present at putting the materials together, and at the examination of the produce. This laborious experiment Mr. Gilpin performed in the same manner, and with the same apparatus, which was used in my own experiments. The method used for introducing air into the bent tube, was that described in the last paragraph of p. 373 in that paper. The soap-lees, like those of my own experiments,

were prepared from salt of tartar, and were of such strength as to yield $\frac{1}{10}$ of their weight of nitre when saturated with nitrous acid. The dephlogisticated air was prepared from turbith mineral, and seemed by the nitrous test to contain about $\frac{1}{15}$ part of phlogisticated air.

On Dec. 6, 1787, in the presence of Sir Joseph Banks, Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. J. Hunter, and Mr. Macie, the materials were put together. The quantity of soap-lees, introduced into the bent tube, was 180 measures, each of which contained 1 gr. of quicksilver; and, as the bore of the tube was rather more than $\frac{1}{3}$ of an inch in diameter, it formed a column of 5 or 6 tenths of an inch in length, which, by the introduction of the air, was divided into 2 parts, one resting on the quicksilver in one leg of the tube, and the other on that in the other leg. The dephlogisticated air was mixed with $\frac{1}{3}$ part of its bulk of atmospheric air of the room in a separate jar, and the reservoir was filled with the mixture; and from it Mr. Gilpin, as occasion required, forced air into the bent tube, to supply the place of that absorbed by means of the electric spark. Hence it appears, that the mixture employed contained a less proportion of common air than that used in either of my experiments. This made it necessary for Mr. Gilpin now and then to introduce some common air by means of the bent tube, whenever from the slowness of the absorption he thought there was too small a proportion of phlogisticated air in the tube. My reason for this manner of proceeding was, that as my first experiment seemed to show, that the dephlogisticated air ought to be in a rather greater proportion to the phlogisticated than the latter did, I was somewhat uncertain as to the proper quantities, and doubted whether I could proportion them in such manner as that it should not be necessary, during the course of the experiment, to add either dephlogisticated or common air. I therefore mixed the airs in such proportion, that I was sure there could be no occasion to add the former; since it was much easier, as well as more unexceptionable, to add common air than dephlogisticated.

On Dec. 24, as the air in the reservoir was almost all used, this apparatus was again filled in the presence of most of the above-mentioned gentlemen, with a mixture of the same dephlogisticated air and common air, in the same proportions as before; and the same thing was repeated on Jan. 19. On Jan. 23, the bent tube was, by accident, raised out of one of the glasses of mercury into which it was inverted, by which it was filled with air, and a good deal of the soap-lees were lost; there was enough however remaining for examination.

On Jan. 28, and 29, the produce of this experiment was examined in the presence of Sir Joseph Banks, Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. Heberden, Dr. J. Hunter, Mr. Macie, and Dr. Watson. It appeared that 9290 measures of the mixed air had been forced into the bent tube from the reservoir.*

* The method of ascertaining the quantity of air forced in was by weighing the reservoir, as mentioned in the above-mentioned paper, p. 374.—Orig.

Besides this, Mr. Gilpin had at different times introduced 872 measures of common air, which makes in all 10162 of air, consisting of 6968 of dephlogisticated air, and 3194 of common air. But as there were 900 measures of air remaining in the tube when the accident happened, the quantity absorbed was only 9262; but this is a much greater quantity than what from my own experiments seemed necessary for this quantity of soap-lees. The soap-lees were poured into a small glass cup, and the tube washed with a little distilled water, in order that as little as possible might be lost. As they were by this means considerably diluted, they were evaporated to dryness; but it was difficult to estimate the quantity of the saline residuum, as it was mixed with a few particles of mercury.

Some vitriolic acid, dropped on a little of this residuum, yielded a smell of nitrous acid, the same as when dropped on nitre phlogisticated by exposure to the fire in a covered crucible; but it was thought less strong. The remainder was dissolved in a small quantity of distilled water, and the following experiments were tried with the solution. It did not at all discolour paper tinged with the juice of blue flowers. It left a nauseous taste in the mouth like solutions of mercury, and most other metallic substances. Paper dipped into it, and dried, burnt with some appearance of deflagration, but not so strongly or uniformly as when dipped in a solution of nitre. The marks of deflagration however were stronger than when the paper was dipped into a solution of mercury in spirit of nitre, but not so strong as when equal parts of this solution and solution of nitre were used. A solution of fixed vegetable alkali, dropped into some of it diluted, produced a slight reddish-brown precipitate, which afterwards assumed a greenish colour. A bit of bright copper being dipped into it, acquired an evident whitish colour, though not so white as when dipped into the solution of mercury in spirit of nitre.

From these experiments it appears, that the mixture of the two airs was actually converted into nitrous acid, only the experiment was continued too long, so that the quantity of air absorbed was greater than in my experiments, and the acid produced was sufficient, not only to saturate the soap-lees, but also to dissolve some of the mercury. The truth of the latter part is proved by the metallic taste of the residuum, its not discolouring the blue paper, the precipitate formed by the addition of fixed alkali, and the white colour given to the copper; and the nitrous fumes produced by the addition of oil of vitriol, as well as the manner in which paper impregnated with the residuum burnt, show as plainly, that the acid produced was of the nitrous kind. It is remarkable however, that during this experiment there were no signs which showed when the soap-lees became saturated. The only time when the diminution proceeded much slower than usual was on Jan. 4. It then seemed to go on very slowly; but as the air

absorbed at that time was only 4830 measures, which is much less than what seems requisite to saturate the alkali, and as the diminution immediately went on again on adding more common air, it seems not likely that the soap-lees were saturated at that time.

On Jan. 10, Mr. Gilpin observed a small quantity of whitish sediment on the surface of the mercury; which seems to show that the soap-lees were then saturated, and that the acid was beginning to corrode the mercury. The quantity of air absorbed was also 6840 measures, which is about as much as I expected would be required. However, as I was persuaded, from the event of my own experiments, that the diminution would either entirely cease, or go on very slowly, as soon as the soap-lees were saturated; and as I was unwilling to stop the experiments before that happened, I thought it best to continue the electrification. On the same morning Mr. Gilpin found, that about 120 measures of the air in the bent tube had been spontaneously absorbed during the night, the quantity therein being so much less than it was the preceding evening, though the electrical machine had not been worked, or any thing done to it during the intermediate time. The reason of this in all probability is, that as the acid was then corroding the mercury, the soap-lees became impregnated with nitrous air, which during the night united to the dephlogisticated air, and caused the diminution.

Though in reality the event of this experiment was such as to establish the truth of my position, that the mixture of dephlogisticated and phlogisticated air is converted by the electric spark into nitrous acid, as fully as if the experiment had been stopped in proper time; yet, as the event was in some measure different from that of my own experiments, and might afford room for cavil, I was desirous of having it repeated; and as Mr. Gilpin was so obliging as to undertake it again, the materials were, on Feb. 11, put together for a fresh experiment, in the presence of most of the above-mentioned gentlemen. The soap-lees employed were the same as before, but 183 measures were now introduced. The dephlogisticated air was different, the former parcel being all used. It was prepared, like the former, from turbith mineral, but was rather purer, as it seemed to contain only $\frac{1}{11}$ of phlogisticated air. The proportion in which it was mixed with common air was that of 22 to 10; so that a greater proportion of common air was now used, in consequence of which it was not necessary for Mr. Gilpin to introduce common air so often.

On Feb. 29, the reservoir was again filled with air of the same kind, in presence of some of the same gentlemen. As it was found by the last experiment that we must not depend on the saturation of the soap-lees being made known by any alteration in the rate of diminution, the process was stopped as soon as the air absorbed was such as from my own experiments I judged sufficient to neutralize the soap-lees. This was effected on the 15th of March. The air

remaining in the tube, when Mr. Gilpin left off working, was 600 measures; but at the time the produce was examined, it was reduced to about 120, so much having been absorbed without the help of any electrification, which is a still more remarkable instance of spontaneous absorption than what occurred in the former experiment. A few days after the experiment began, a black film was formed in one of the legs, which I suppose must have been a mercurial ethiops; but whether owing to some small degree of foulness in the mercury or tube, or to any other cause, I cannot tell. This foulness seemed not to increase; but on March 10, when the air absorbed was about 5200, a whitish sediment began to appear on the surface of the mercury.

On March 19, the produce was examined in the presence of Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. Heberden, Dr. J. Hunter, Mr. Macie, and Dr. Watson. The mixed air forced into the bent tube from the reservoir was 6650 measures, besides which Mr. Gilpin had at different times introduced 630 of common air, which makes in all 7280, containing 4570 of dephlogisticated, and 2710 of common air. The soap-lees were evaporated to dryness as before. The residuum weighed 2 gr., but there were 2 or 3 globules of mercury mixed with it, which might very likely weigh $\frac{1}{2}$ gr. This being dissolved in a small quantity of water, the following experiments were made with it. It did not at all discolour paper tinged with blue flowers. Slips of paper were dipped into it, and dried; and, by way of comparison, other slips of paper were dipped into a solution both of common nitre and phlogisticated nitre, and also dried. The former burnt in the same manner, and with as strong marks of deflagration, as the latter. It had a strong taste of nitre, but left also a slight metallic taste on the tongue. It did not give any white colour to a piece of clean copper put into it.

In order to see whether the whitish sediment, which was before said to be formed in the bent tube, contained any mercury, the remainder of this solution was diluted with some more distilled water, and suffered to stand till the white sediment had subsided. The clear liquor being then poured off, the remainder, containing the sediment, which seemed to amount only to a very small quantity, was put on a piece of bright copper, and dried on it; a piece of clean gold was then laid over it, and both were exposed to heat. Both metals acquired a whitish colour, especially the gold, but which was very indeterminate. In order to discover how nice a test of alcalinity the paper tinged with blue flowers was, a saturated solution of common nitre was mixed with $\frac{1}{110}$ of its bulk of the soap-lees; and this mixture was found to turn the paper evidently green; so that, as the solution of nitre contains about twice as much alkali as the soap-lees, it appears, that if the residuum had wanted only $\frac{1}{110}$ part of being saturated, it would have discoloured the paper.

From the foregoing trials it appears, that the mixture of dephlogisticated and

common air in this experiment was actually converted into nitrous acid, and was sufficient not only to saturate the soap-lees, but also to dissolve some of the mercury. The quantity dissolved however was very small, and not sufficient to diminish sensibly the deflagrating quality of the nitre; so that the proof of the air being converted into nitrous acid was as evident as if no mercury had been dissolved. In this experiment, as well as the former, no indication of the soap-lees becoming saturated was afforded by any cessation in the diminution of the air; whereas in my experiments it was very manifest. I do not know what this difference should be owing to, except to Mr. Gilpin's giving much stronger electrical sparks than I did. In his experiments the metallic knob which received the spark, and conveyed it to the bent tube, was usually placed at about $2\frac{1}{4}$ inches from the conductor, so that the spark jumped through $2\frac{1}{4}$ inches of air, in passing from the conductor to the knob, besides from $1\frac{1}{4}$ to $2\frac{1}{4}$ inches of air in the tube; whereas in my experiments, I believe, the knob was never placed at the distance of more than $1\frac{1}{4}$ inch from the conductor, and the quantity of air in the tube was much less; but the conductor and electrical machine were the same.

Except this, the only difference that I know of in the manner of conducting the experiment is, first, that Mr. Gilpin usually continued working the machine for $\frac{1}{2}$ an hour at a time, whereas I seldom worked it more than 10 minutes; and 2dly, that in Mr. Gilpin's experiments the common air in the reservoir bore a less proportion to the dephlogisticated air than in mine; in consequence of which it was necessary for him frequently to introduce common air. On this account, the proportion of the 2 airs in the bent tube would be considerably different at different times; but on the whole, the common air absorbed bore a greater proportion to the dephlogisticated than in mine.

Though the whole quantity of air absorbed in these experiments is known with considerable precision, yet it is impossible to determine, with any accuracy, how much of each kind was absorbed, on account of our uncertainty about the nature of the air which remained at the end of the experiment. But if in the last experiment we suppose that the air absorbed spontaneously between the 15th and 19th of March was entirely dephlogisticated, and that what remained at the end of that time was of the purity of common air, it will appear, that 4090 of dephlogisticated and 2588 of common air, which is equivalent to 4480 of pure dephlogisticated air and 2198 of phlogisticated air, were absorbed at the time the electrification was stopped, and consequently the dephlogisticated air is $\frac{204}{100}$ of the phlogisticated air; whereas in my first experiment it seemed to be $\frac{185}{100}$, and in my last $\frac{263}{100}$. But the quantity of acid produced, and consequently I suppose the saturation of the soap-lees, depends only on the quantity of phlogisticated air absorbed; and the effect of the greater or less quantity of dephlogisticated air is only to make the nitre produced more or less phlogisticated. Now in this

experiment the bulk of the phlogisticated air was $12\frac{2}{10}$ that of the soap-lees. In my first experiment it was $11\frac{9}{10}$, and in my last $10\frac{8}{10}$.

As many persons seem to have supposed that the diminution of the air in these experiments is much quicker than it really is, though I do not know any thing in my paper which should lead to suppose that it was not very slow, it may be proper to say something on this head. As the quickness of the diminution depends so much on the power of the electrical machine, I can only speak as to what happens with the machine used in these experiments. This was one of Mr. Nairne's patent machines, the cylinder of which is $12\frac{1}{2}$ inches long, and 7 in diameter. A conductor of 5 feet long, and 6 inches in diameter, was adapted to it, and the ball which received the spark was placed at 2 or 3 inches from another ball, fixed to the end of the conductor. Now, when the machine worked well, Mr. Gilpin supposes he got about 2 or 300 sparks a minute, and the diminution of the air during the half hour which he continued working at a time, varied in general from 40 to 120 measures, but was usually greatest when there was most air in the tube, provided the quantity was not so great as to prevent the spark from passing readily.

The only persons I know of, who have endeavoured to repeat this experiment, are, M. Van Marum, assisted by M. Paets Van Trootswyk; M. Lavoisier, in conjunction with M. Hassenfratz; and M. Monge. I am not acquainted with the method which the 3 latter gentlemen employed, and am at a loss to conceive what could prevent such able philosophers from succeeding, except want of patience. But M. Van Marum, in his Première Continuation des Expériences, faites par le moyen de la Machine électrique Teylerienne, p. 182, has described the method employed by him and M. Van Trootswyk. They used a glass tube, the upper end of which was stopped by cork, through which an iron wire was passed, and secured by cement, and the lower end was immersed into mercury; so that the electric spark passed from the iron wire to the soap-lees. After so much of a mixture of 5 parts of dephlogisticated and 3 of common air as was equal to 21 times the bulk of the soap-lees * was absorbed, some paper was moistened with the alkali, which by its burning appeared to contain nitre, but showed that the alkali was not near saturated. The experiment was then continued with the same soap-lees till more of the air, equal to 56 times the bulk of the soap-lees, was absorbed, which is near double the quantity required to saturate them; but yet the diminution went on as fast as ever. It was then tried, by the burning of paper dipped into them, how nearly they were saturated; but they still seemed far from being so.

The circumstance of using the iron wire appears evidently objectionable, on account of the danger of the iron wire being calcined by the electric spark, and

* This is rather more than half of that requisite to saturate the soap-lees.—Orig.

absorbing the dephlogisticated air; and when I first read the account, I thought this the most probable cause of the difference in the result of our experiments; but I am now inclined to think that the case was otherwise. From the manner in which M. Van Marum expresses himself, it seems that the only circumstance, from which they concluded that the alkali was not saturated, was the imperfect marks of deflagration that the paper dipped into it exhibited in burning; which, as we have seen, might proceed as well from some of the mercury having been dissolved as from the alkali not being saturated. I am much inclined to think therefore, that, so far from the soap-lees not having been saturated, the quantity of acid produced was in reality much more than sufficient for this purpose, and had dissolved a good deal of the mercury; for the quantity of air absorbed favours this opinion, and the phenomena agree well with Mr. Gilpin's first experiment, in which this was certainly the case; whereas, if the diminution had proceeded chiefly from the dephlogisticated air being absorbed by the iron, the tube towards the end of the experiment would have been filled chiefly with phlogisticated air, which would have made the diminution proceed much slower than before; but we are told that it went on as fast as ever. It is most likely therefore, that the apparent disagreement between their experiment and mine, proceeded only from their having continued the process too long, and from their not having properly examined the produce.

M. Van Marum then proceeds to say: "Surpris de cette différence de résultat, j'envoyai une description exacte de nos expériences à M. Cavendish, le priant en même tems de m'instruire s'il pourroit trouver la cause de cette différence; et comme la seule différence essentielle, par laquelle notre expérience différoit de celle de M. Cavendish, consistoit en ce que nous avons employé de l'air pur produit du précipité rouge ou du minium, au lieu de l'air pur produit de la poudre noire formée par l'agitation du mercure avec le plomb, dont M. Cavendish ne donne pas la manière de le produire,* je le priai de me communiquer de quelle manière il étoit venu a cet air, parceque je desirois de répéter l'expérience avec ce même air: mais comme il ne m'a fourni aucune élucidation sur la cause vraisemblable de la différence du resultat de nos expériences, et qu'il ne lui a pas plu de me communiquer sa manière de produire l'air pur qu'il avoit employé pour ses expériences, m'écrivant, qu'il s'étoit proposé d'en parler dans un écrit public, la longueur ennuyante de ces expériences nous a fait prendre la resolution de différer leur continuation, pour obtenir une parfaite saturation de la lessive, jusqu'à

* The using the iron wire formed a material difference in our manner of conducting the experiment, and one which may perhaps have had great influence on the result; but I do not see how the using some other kind of dephlogisticated air, instead of that prepared from Dr. Priestley's black powder, can in the least degree form an essential difference, as in the same paragraph in which I mention my having used this kind of air in my first experiment, I say, that in my second experiment I used air prepared from turbith mineral.—Orig.

ce que M. Cavendish ait publié sa manière de produire l'air pur, dont il s'est servi; nous contentant pour le present d'avoir vu, que l'union du principe d'air pur et de la mofette produit le l'acide nitreux, suivant la découverte de M. Cavendish."

As I should be sorry to be thought to have refused any necessary information to a gentleman who was desirous to repeat one of my experiments, and who by his situation was able to do it with less trouble than any one else, I hope the society will indulge me in adding a copy of my answer, that they may judge whether this is in any degree a fair representation of it.

" *To M. Van Marum.*

" Sir,—I received the honour of your letter, in which you inform me of your ill success in trying my experiment on the conversion of air into nitrous acid by the electric spark. It is very difficult to guess why an experiment does not succeed, unless one is present and sees it tried; but if you intend to repeat the experiment, your best way will be to try it with the same kind of apparatus that I described in that paper. If you do so, and observe the precautions there mentioned, I flatter myself you will find it succeed. The apparatus you used seems objectionable, on account of the danger of the iron being corroded by absorbing the dephlogisticated air. As to the dephlogisticated air procured from the black powder formed by agitating mercury mixed with lead, as it was foreign to the subject of the paper, and as I proposed to speak of it in another place, I did not describe my method of procuring it. As far as I can perceive, the success depends entirely on carefully avoiding every thing by which the powder can absorb fixed air, or become mixed with particles of an animal or vegetable nature, or any other inflammable matter: for which reason care should be taken not to change the air in the bottle in which the mercury is shaken, by breathing into it as Dr. Priestley did, or even by blowing into it with a bellows, as thereby some of the dust from the bellows may be blown into it. The method which I used to change the air was, to suck it out by means of an air-pump, through a tube which entered into the bottle, and did not fill up the mouth so close but what air could enter in from without, to supply the place of that drawn out through the tube.—I am, &c."

With regard to the main experiment, it was not in my power to give him further information than I did; as I pointed out the only circumstance to which, at that time, I could attribute the difference in our results. And with regard to the manner of preparing the dephlogisticated air from the black powder, I have mentioned all the particulars in which my manner of proceeding differed from Dr. Priestley's, and have also explained on what I imagine the success entirely depends; so that I believe no one, at all conversant in this kind of experiments, will think that I did not communicate to him my method of procuring that air.

XVIII. Experiments on the Effect of various Substances in Lowering the Point

of Congelation in Water. By Chas. Blagden, M. D., Sec. R. S. and F. A. S. p. 277.

The experiments necessary to determine what effect the admixture of various substances would produce on the property of water to be cooled below the freezing point, naturally led to a more particular consideration of the power of such admixtures in making water require a greater degree of cold before it congeals. Many curious questions occurred on this subject, which could only be answered by fresh experiments. These were made nearly in the same manner as the preceding; that is, the liquor, whose freezing point was meant to be tried, was put into a glass tumbler, to the height of 2 or 3 inches above the bottom, and the tumbler was then immersed in a frigorific mixture of common salt and ice or snow.

The first object of investigation was the ratio according to which equal additions of the same substance depress the freezing point. Dr. B. began with common salt, of the purest kind. This salt he dissolved in distilled water, in various proportions, and found the corresponding points of congelation to be as expressed in the annexed table; where the first column indicates the number of parts and decimals of water to one part of the salt, and the 2d column shows the freezing point found by the experiment. It appeared clearly, on comparing the proportions of water to salt, with the corresponding number of degrees which the freezing point was reduced below 32°, that the effect of the salt was nearly in a simple ratio; namely, that if the addition of a 10th part of salt to the water sunk the freezing point about 11°, or to 21°, it would be depressed double that quantity, or to 10° nearly, when a 5th part of salt was dissolved in the water. To show therefore how far this simple proportion is exact, a third column is added to the table, which is made by selecting the lowest freezing point that was obtained without ambiguity in the experiment, and calculating, by a simple inverse proportion, what all the other points should have been according to that ratio. Thus, when a 4th part of its weight of common salt was dissolved in water, the freezing point of the liquor was 4°; therefore, to determine what it should be when only $\frac{1}{32}$ part of salt was added to the water, the formula is 32 : 4 :: 28 (the number of degrees that the point 4° is below the freezing point of pure water) : 3½; which subtracted from 32° gives 28°½ for the freezing point of that solution. All the rest of the 3d column of the table is found in the same manner, and with very little trouble, because $4 \times 28 = 112$ is a constant number, which being divided by the numbers of the first column, the quotient is the number of degrees sought. In all the experiments, none but distilled water was employed.

Common salt.

Proportion of water to the salt.	Freezing point by the experiment	Freezing point by calculation.
32 : 1	29°	28½°
32 : 1	28 +	28½
24 : 1	27½	27⅓
16 : 1	25¼	25
10 : 1	21⅛	20¾
7.8 : 1	18½	17⅔
6.2 : 1	13½	14
5 : 1	9½	9⅗
5.5 : 1	7⅕	7
4 : 1	4	4

The numbers in the 3d column of the table come so near to those in the 2d, that most likely the small differences between them ought to be ascribed to errors in the experiments; whence we should conclude, that the salt lowers the freezing point in the simple inverse ratio of the proportion which the water bears to it in the solution. This solution was in one instance cooled 8¼, and in several 5 or 6°, below its freezing point; but in general it shot rather more readily than some other solutions; which he ascribed, from the analogy of his former experiments, to its less transparency. This salt with snow, in the manner of frigorific mixtures, produced a cold of — 4°.

Nitre.

The next salt which was tried for its effect in lowering the freezing point of water, was nitre. It was part of a large compound crystal, or bundle of crystals, apparently very pure, such as is used in manufacturing the best gunpowder. This being mixed with distilled water, in different proportions, the solutions froze according to the annexed table. The 3d column is calculated from the 5th experiment, in which the freezing point of a solution of one part of salt in eight of water proved to be 26°.

Proportion of water to the salt.	Freezing point by the experiment.	Freezing point by calculation.
32 : 1	30½°	30½°
24 : 1	30	30
16 : 1	28¾	29
10 : 1	27	27¼
8 : 1	26	26
7.9 : 1	26½	salt deposited.
7 : 1	26½	salt deposited.
6.85 : 1	27	much salt deposited.

Nitre is well known to differ from common salt in being much more soluble in warm than in cold water. Hence it would be nothing remarkable, that the solutions being made in water above the freezing point, some of the salt should, when they exceeded a certain strength, be deposited before they began to freeze. But a further question occurred here, whether, when a solution was cooled below its freezing point, the salt would still continue to be deposited; or whether it would not have parted with all the salt it was obliged to let go by the time it came to the degree at which it was to freeze, and would retain the remainder notwithstanding any subsequent cooling. To determine this, Dr. B. noticed carefully the quantities of salt deposited at the bottom of the tumbler, in comparison with the cold of the solution as shown by the immersed thermometer; and he found, that in some cases (for instance, when the salt was to the water only as 1 : 10) the deposition did not begin till after the solution had passed its freezing point; and that when it began earlier, still there was no stop at the freezing point, but the quantity continued augmenting as the cold of the solution proceeded, and that rather in an increasing ratio. Thus when the saturated solution was cooled 8 or 10° below its freezing point, which often happened, the collection of nitre at the bottom was very great; and in this manner he could render a saturated solution of nitre no longer saturated when it came to freeze, the deficiency being sometimes so great as to raise the point of congelation a

degree or more. Hence was ascertained the unexpected fact, that the lower such solutions are cooled, the higher is their freezing point.

The nitre deposited by the solution as it cooled, formed, if the vessel remained at rest, small but very white and compact prismatic or needle-like crystals, of considerable length, pointing different ways, and at last curiously interwoven with each other. But if these were broken down, or the solution was stirred with any force, the remaining nitre deposited itself in such minute crystals as to have much the appearance of a powder; probably from the destruction of the regular surfaces on which it would otherwise have continued to form. Frequently, in the stronger solutions, there appeared near the bottom and side of the tumbler many elegant stellated crystals, perhaps a quarter of an inch in diameter, all separate, but sometimes crouding very close on each other, so as to exhibit a spectacle of much beauty. The ice of solutions of nitre, especially when it began to thaw, was very different from common ice, having a soft woolly appearance, as if of a more tender and loose texture. Something of the same kind was observable in the ice of all the other solutions, sufficiently distinguishing it from any that can be formed of pure water. All the solutions of nitre were remarkably limpid, having no tendency to an opaque or turbid cast; and accordingly they were very easily cooled below the freezing point, and could not but with difficulty be made to shoot till they had passed it many degrees. In 2 instances they cooled more than 10°; namely, a solution of 1 part of nitre in 24 of water cooled slowly to $19\frac{1}{2}$°, and then shooting, the thermometer came up to 30°; and another solution, in which the nitre was to the water as 1 : 10, cooled rather below 16°, and having produced some stellated crystals, rose, when the perfect congelation took place, up to 27°.

As, when pure water is cooled below its freezing point, the least particle of ice or snow brought into contact with it causes an instant congelation, Dr. B. was curious to know whether the same effect would be produced when salts were dissolved in the water. Therefore, having one of these nitrous solutions, whose proportions were 8 : 1, he cooled it to 24°, about 2° below its freezing point, and then, no salt being deposited, he put into it a small bit of ice. The effect of this was not instantaneous, as in pure water, though ultimately the same; the bit of ice gradually enlarged, and when it was stirred about in the liquor, a number of star-like crystals formed, which being scattered through it soon brought it to a uniform temperature of 26°. This same solution, when cooled in a preceding experiment to 18°, had its freezing point at 27°, from the quantity of nitre that had been deposited. In all solutions therefore, of such salts as are much more soluble in hot than in cold water, if it be desired to find their freezing point when they are loaded with as much of the salt as the water can contain at that temperature, te most effectual method is to oblige them to

shoot, as soon as they can be made to do so, by putting in a small bit of ice or snow; for thus the fallacy which might otherwise arise from the deposition of some of the salt will be avoided. A doubt having been suggested, whether the contact of a crystal of salt might not also bring on the congelation, that experiment was tried, but it produced no effect. Indeed, the formation of saline crystals in these experiments, the liquor still remaining fluid, was a sufficient proof to the contrary. On the whole it seems evident from the preceding table, that the effect of nitre, like that of common salt, is to depress the freezing point in the simple ratio of its proportion to the water; which will be found universally true when allowance is made for the deposition and other sources of fallacy before enumerated. This nitre produced, with snow, a cold of between 26° and 27°.

Sal Ammoniac.

As nitre sunk the freezing point of water so little, namely, but 6°, Dr. B. had recourse for the next set of experiments to that neutral salt which, after sea salt, produces the greatest cold with ice; which is, the common sal ammoniac. The different solutions of this salt in water, being submitted to the action of the frigorific mixtures, froze according to the following table.

Proportion of water to the salt.	Freezing point by the experiment.	Freezing point by calculation.
15.7 : 1	24½°	24⅓°
10 : 1	20½	20
9.8 : 1	20	19¾
7.9 : 1	16½	16¾
6 : 1	12	12
5 : 1	8	8
4 : 1	4	salt deposited.

The third column is calculated from the last experiment but one, in which the freezing point of a solution of 1 part of the sal ammoniac in 5 of water proved to be 8°. In this table also the numbers of the 3d column agree sufficiently with those of the 2d to show, that sal ammoniac, like the 2 preceding salts, depresses the freezing point in the simple ratio of the proportion in which it is mixed with the water.

It has been a question much contested, whether saline solutions deposit their salt on freezing. That some separation, or a tendency to separation, takes place, many facts concur to prove; and among the rest some phenomena observed in the above-mentioned experiments. For instance, the stellated crystals, when first formed, were barely suspended in the water, and sometimes they even gradually subsided to the bottom; which shows, that they consisted of salt chiefly, only inviscated with ice, or at at least of an over proportion of salt: for the principal mass of ice formed in a saturated solution floats in it like common ice in pure water. Sometimes in solutions of sal ammoniac, and such other salts as separate by the cooling of the water, a sort of flocculent substance is formed, which subsides in the water, and is thus distinguished from the proper ice of the solution, which it otherwise much resembles in appearance. It is composed chiefly of the deposited salt, in very minute crystals like powder, inviscated and kept together with a little ice. The sal ammoniac, mixed with snow, produced a cold of from 4° to 4½° of Fahrenheit's scale.

Rochelle Salt.

Of all the solutions submitted to these experiments, there were none more transparent and elegant than those made with Rochelle salt. The water dissolved a large proportion of this substance, and had its freezing point sunk according to the following table.

Proportion of water to the salt.	Freezing point by the experiments.	Freezing point by calculation.
10 : 1	29½°	29¾°
5 : 1	27½	27½
4 : 1	26⅓	26½
2.6 : 1	24	23½
2.25 : 1	22½	22¼
2 : 1	21	21
1.6 : 1	24	salt deposited.

The 3d column is calculated from the last experiment but one, in which the freezing point of a solution of 1 part of the Rochelle salt in 2 parts of water proved to be 21°. All the solutions of Rochelle salt bore to be cooled remarkably well. In one instance the liquor sunk 11°½ below its freezing point; namely, the solution of 1 part of the salt in 5 of water, whose freezing point proved 27°½, and which cooled to 16° before the crystals of ice shot. In 2 other instances it sunk fully 9° below its freezing point. In trying the greatest cold to be obtained by mixing Rochelle salt with snow, the thermometer could not be got lower than 24°.

Glauber's salt was likewise subjected to the experiments; but its utmost effect in producing cold with snow appearing to be only 2°, this was too small a scale for settling any thing as to the ratio. A solution of it in water, in the proportion of 1 : 5, cooled readily to 31°; but the salt was deposited in great quantities, and often so fast as to stop the cooling of the bottom of the liquor entirely, though the vessel was immersed in a strong frigorific mixture. This phænomenon is exactly the converse of the cold produced by dissolving salts in water; for as there some heat is absorbed, and becomes latent, by the change of the salt from a solid to a fluid state, so here some heat is evolved as the salt assumes the solid crystalline form. The effect is so much more manifest with Glauber's salt only, because the formation of the crystals proceeds so rapidly; whence the quantity of heat generated equals or exceeds the cold communicated by the freezing mixture. Some odd appearances are produced by this sudden stop of the cooling, and the rapid deposition of salt; for instance, a particular ebullition in certain parts of the liquor; but any intelligible description of them would be too minute.

These were all the salts with an alkaline basis that were tried. They all agreed as to the chief object of these experiments, namely, to determine how much the freezing point of water would be sunk by dissolving them in it in various proportions; which by these experiments appears to be, as nearly as could be determined, according to the simple ratio of the proportion each salt bears to the water. Dr. B. now resolved to try a few salts with an earthy and metallic basis.

Sal Catharticus Amarus.

The common sal catharticus amarus of the shops was the specimen used of an earthy salt. It formed a turbid inelegant solution, as if dirty; and with various proportions of water produced the following points of congelation.

Proportion of water to the salt.	Freezing point by the experiment.	Freezing point by calculation.
16 : 1	31°	31°
10 : 1	30	$30\frac{1}{2}$
4 : 1	$28\frac{1}{4}$	28
3 : 1	$26\frac{4}{5}$	$26\frac{4}{5}$
2.4 : 1	$25\frac{1}{2}$	$25\frac{1}{2}$

The 3d column is calculated from the last experiment, where the freezing point of a solution of 1 part of the sal catharticus amarus in 2.4 of water proved to be $25\frac{1}{2}$°. No salt was deposited from the strongest of these solutions; and as that here used was a deliquescent salt, it must probably have been in a vast proportion to the water, before any such effect would have taken place. Dr. B. has sunk a thermometer with it and snow to 7°$\frac{1}{2}$; which according to the proportions in the table, would make more than 3 parts of the salt to 2 of water. Accordingly, a large quantity of the salt was required to the snow. No particular phænomenon was observed with this salt, except the singular configuration of its ice, which assumed the form of fungi, or of some kinds of lichen, with feathered striæ. The solutions were difficult to cool much below their freezing point.

Green Vitriol.

Of the salts with a metallic basis, green vitriol affords one of the most transparent solutions in water. It sinks the thermometer nearly to $27\frac{1}{2}$° with snow, and reduced the freezing point of water according to the following table.

Proportion of water to the salt.	Freezing point by the experiment.	Freezing point by calculation.
10 : 1	$30\frac{3}{4}$°	31°
6 : 1	$30\frac{1}{4}$	$30\frac{1}{2}$
4 : 1	$29\frac{3}{4}$	$29\frac{1}{2}$
3 : 1	$28\frac{3}{4}$	$28\frac{3}{4}$
2.4 : 1	28	28

The 3d column is calculated from the last experiment, in which the freezing point of a solution of 1 part of the green vitriol in 2.4 of water proved to be 28°. The ice formed by these solutions assumed a foliaceous configuration, with a texture of penniform striæ, in some respects like the appearance exhibited by a drop evaporating under a microscope, as delineated by Baker. Scarcely any salt gave the point of congelation so regularly in the proportion of the quantities mixed with the water, and none afforded solutions which cooled more easily and readily below the freezing point. In 2 instances the cooling was more than 11°.

White Vitriol.

Having found that white vitriol, mixed with snow, produced a cold of 20°, melting the snow remarkably fast, I was induced to try the freezing point of its solutions. But though it dissolved very readily in water, yet the liquor it formed was so turbid and thick, that little satisfaction could be derived from the

experiments. The only numbers to be relied on are the following, which agree sufficiently with the general result.

Proportion of water to the salt.	Freezing point by the experiment.	Freezing point by calculation.
10 : 1	31°	31°
5 : 1	29½	30
3 : 1	28¾	28¾

The 3d column is calculated from the last experiment, in which the freezing point of a solution of 1 part of the white vitriol in 3 of water proved to be 28¾°. These solutions cooled very ill, none of them having sunk much below the freezing point, and the strongest, which had a copious sediment, forming a crust of ice at the bottom of the tumbler, before it was reduced at all below the term of congelation.

M. Achard, of Berlin, having alleged*, that borax, instead of raising the boiling point of water, like other saline substances, very sensibly depresses it, Dr. B. determined, however extraordinary the fact might appear, to try whether it had any peculiar effect on the freezing point. But having made the experiment with nearly a saturated solution of borax, the thermometer when it congealed was evidently below 32°: he believes about a degree.

As a neutral or middle salt, which when crystallized is always nearly of the same nature, and dissolves in a regular proportion in water, seemed likely to afford the most simple case of the effect of extraneous admixtures, it was with such that he began these experiments. But having found that with them the simple ratio prevailed, he proceeded to try substances of a more variable nature, and capable of being mixed with water in almost any proportion; such as acids, alkalis, and ardent spirits. A material difference in the law, which seemed to occur in these new experiments, renders it proper to defer the account of them till some reflections on the preceding facts, with a few additional experiments to which they gave rise, have been premised.

It seems now universally allowed, that in frigorific mixtures the melting of the snow or ice is the principal cause of the cold produced; all that heat which must become latent in order to give water its fluent form being taken from the sensible heat of the ingredients. But as, when crystallized salts are employed for the purpose, these also are reduced to a liquid form, there must, from this circumstance, be some additional cold produced, such for instance as would be occasioned by dissolving the same salt in water. Suppose then that the latent heat of water is 150°, and that sal ammoniac, in dissolving to saturation, produces so much cold as sinks the whole solution about 20°; it is evident, that if this salt and ice are mixed together in such proportions as just to melt each other, the total cold generated in the operation must amount to 170°. And so much actually is produced before the whole liquefaction is effected; and yet a mixture of these 2 substances will sink the thermometer no lower than to 4° of Fahrenheit's.

* See Crell's Chem. Annalen, 1786, vol. 1, p. 501.

scale. The consideration of this apparent difficulty has led to the supposition, that a certain quantity of fire is contained in the crystals of the salt, which being disengaged in the solution keeps up the mixture to a certain temperature. But Dr. B. conceives that the phenomenon depends simply on the gradual liquefaction of the ingredients, a necessary consequence of the cold produced. A saturated solution of sal ammoniac freezes itself at 4°; therefore, when the mixture is reduced by the liquefaction of the ingredients, to that temperature, no more of them can melt, because any addition of cold would freeze what is already melted; and if the mixture, under such circumstances, were placed in an atmosphere of its own temperature, the ingredients would remain for ever in that same state, without any further liquefaction. But in an atmosphere warmer than 4°, they continue to melt, more or less slowly, as the heat which is gradually communicated furnishes what is necessary to become latent. This communicated sensible heat being immediately converted into latent, the mixture will always be kept down to the same temperature as long as there is a sufficient mass of unmelted materials; and it can sink no lower, because then the liquefaction would be stopped; consequently such mixtures must preserve, as they have been found to do, a pretty uniform temperature, so as to have been formerly used for graduating thermometers. And the whole cold produced, or, to speak properly, the whole of the heat made to disappear, he presumes to be ultimately equal to the full quantity of latent heat belonging to the dissolved ice and salt.

As it is well known that water, after it has been saturated with one salt, will take up a certain portion of another salt without depositing any of the former, Dr. B. was curious to try what effect the addition of this 2d salt would produce on the freezing point; and particularly whether it would depress the freezing point of the saturated solution the same number of degrees that an equal proportion of the same salt would depress the freezing point of water; and whether the same simple ratio would hold good, or any new law take place. To bring this to the test of experiment, he took a saturated solution of nitre, whose freezing point of course was between 26° and 27°; and adding to it the purified common salt in various proportions, he obtained the following results.

Compound solution of nitre and common salt.

Proportion of water to the nitre.	Proportion of water to the common salt.	Freezing point by the experiment.	Freezing point by calculation.	Difference.
	30.2 : 1	23½°	22¾°	¾°
A saturated solution.	15 : 1	20¾	19	1¾
	10 : 1	17½	15¼	2¼
	7.4 : 1	13½	11½	2
	5 : 1	5¼	4	1¼

It is evident, from the freezing points of this compound solution, that the com-

common salt depressed the freezing point of the solution of nitre something less than it would have depressed the freezing point of water, if added to it in the same proportion. To show this more evidently, he added a 4th and a 5th column to the table: the 4th column is formed by taking the freezing point of the saturated solution of nitre as $26\frac{1}{4}$°, and then finding how many degrees the quantity of common salt added would have depressed the freezing point of water; this number of degrees subtracted from the constant number $26\frac{1}{4}$°, gives the freezing point by calculation, namely, what it should have been if the salt had produced the same effect on the solution of nitre as it would on pure water; and the difference between this and the freezing point found by the experiment gives the numbers in the 5th column. From the table it is apparent, that the deficiency of effect from the salt goes on increasing to the 3d experiment, after which it decreases. Probably some particular law takes place, which it would require a great number of experiments to develope; but the decrease toward the last may in part be owing to the greater quantity of nitre which the water, when it began to be loaded with common salt, retained at the time of congelation, and which must have its effect in depressing the freezing point. The above-mentioned circumstance seems rather contradictory to an opinion which has been entertained, that when one salt, added to a saturated solution of another salt, enables it to take up more of the former salt, it is only because the water of crystallization of the 2d salt really adds to the quantity of the dissolving fluid.

Dr. B. next proceeded to try a similar experiment with sal ammoniac and the purified common salt, but with this difference, that neither salt should be added to the water in such quantity as to come near the point of saturation, suspecting that the diminution of effect observed in the foregoing experiments might depend, in part at least, on this circumstance. The sal ammoniac therefore was dissolved in water in the proportion of 1 : 10, and the corresponding point of congelation appeared by experiment to be $20\frac{1}{4}$°, agreeing very well with the table of sal ammoniac formerly given. To this solution was added the purified common salt, in proportion to the water as 1 : 15, and then as 1 : 10; the resulting points of congelation were as shown in the following table, constructed in all respects as the immediately preceding.

Compound solution of sal ammoniac and common salt.

Proportion of water to the sal ammoniac.	Proportion of water to the common salt.	Freezing point by the experiment.	Freezing point by calculation.	Difference.
10 : 1	15 : 1	$12\frac{3}{4}$°	13 °	$+\frac{1}{4}$
10 : 1	10 : 1	$9\frac{1}{4}$	$9\frac{1}{5}$	$-\frac{1}{20}$

Hence it appears, that in this compound solution both salts produced, as exactly as the experiments can be expected to show, their full effect in depressing

the point of congelation. When the solutions at length froze, after cooling many degrees below the freezing point, the crystals shot in a very beautiful manner round the bulb and up the stem of the thermometer.

In a compound solution of Rochelle and common salt there was however a deficiency of effect. For the solution of Rochelle salt in the proportion of 1 part to 4 of water, having its freezing point at $26\frac{1}{3}°$; when common salt was dissolved in it, in the proportion of $\frac{1}{10}$th, the freezing point appeared by experiment to be $16\frac{1}{4}°$, whereas by calculation it should have been depressed nearly to 15°.

A composition of 3 salts was affected as follows:

Compound solution of Rochelle salt, common salt, and sal ammoniac.

Proportion of water to the Rochelle salt.	Proportion of water to the common salt.	Proportion of water to the sal ammoniac.	Freezing point by the experiment.	Freezing point by calculation.	Difference.
9.8 : 1	10 : 1	17 : 1	13° —	$11\frac{1}{2}°$	— $1\frac{1}{2}°$

The computation is made thus: Rochelle salt, in the proportion of 1 : 9.8, depresses the freezing point $2\frac{1}{4}°$; common salt, in the proportion of 1 : 10, sinks it $11\frac{1}{4}$; and sal ammoniac, in the proportion of 1 : 17, sinks it 7°; now $2\frac{1}{4} + 11\frac{1}{4} + 7 = 20\frac{1}{2}$; which subtracted from 32, leaves $11\frac{1}{2}°$ for the computed freezing point of this mixture. The moment Dr. B. had found by experiment, that the addition of a different salt to the saturated solution of any salt, would still further depress its freezing point, it was obvious to conclude, that greater cold could be produced with snow by a mixture of salts than by means of either taken separately. He made several experiments with this view, and found it uniformly the fact, that by adding a certain proportion of a salt which had less power of producing cold with snow, to one which had a greater power, the frigorific effect of the latter was sensibly increased. Passing over examples of less consequence, it may be sufficient to instance common salt and sal ammoniac. The ordinary common salt he used to mix with snow, sunk the thermometer to — 5°; the sal ammoniac to + 4°; but when some of the latter salt was mixed with the former, the composition produced with snow a cold of — 12°. On several occasions he made use of this composition to obtain a greater degree of cold than common salt alone would produce, and found it a very convenient method. On this principle it is that impure common salt always makes a stronger freezing mixture than the pure; it being, in fact, a composition of salts. Three salts have produced a greater cold than 2, but he had not carried the experiments far enough to ascertain the limits of this effect.

As the cold produced by common salt with snow is — 4° or more, and by sal ammoniac + 4°, it is difficult to conceive in what manner Fahrenheit fixed the zero of his thermometer. All those who have examined the few authentic pas-

sages to be found in authors on this subject, will be sensible how vaguely they are expressed, leaving it doubtful whether he used common salt alone, sal ammoniac alone, or both mixed together. There is no method of sinking a thermometer exactly to 0° with these salts and snow, but by means of a certain smaller proportion of common salt added to the sal ammoniac; and it would have been an extraordinary chance, that Fahrenheit should hit precisely this proportion often enough to make him rely on the point so found as the commencement of his scale; more especially as the proportion is probably no greater than a 7th or a 6th of common salt to the sal ammoniac. Is it possible that Fahrenheit, finding a considerable difference in his experiments, took the mean between them for his zero, without any respect to the different nature of the salts with which he operated? It appears that he was at this time so little acquainted with the subject, as to consider his zero as the utmost limit of cold.

Dr. B. comes now to certain substances which, by equal additions, seem to depress the freezing point of water in an increasing ratio. These, as was mentioned before, are acids, alkalis, and spirit of wine. The specific gravity of the vitriolic acid employed was 1.837 at 62° of heat; its effect on the freezing point is shown by the annexed table.

Vitriolic acid.

Proportion of water to acid.	Freezing point by the experiment	Freezing point by calculation.
10 : 1	24½°	22¼°
5 : 1	12½	12⅖
4 : 1	7½	7½

This table is constructed in the same manner as those formerly given of the solution of simple salts; the last experiment, where the proportion is 4 : 1, being taken as the standard for computation; and the extreme difference between the calculation and experiment is no less than 2¼°, on a reduction of the freezing point from 24½° to 7½°. The freezing point, set down in the table, is that to which the liquor rose on congealing, after having been cooled several degrees lower; which it is proper to remark, because the ice rose 2 or 3 degrees in thawing.

The nitrous acid employed was smoking, and had its specific gravity 1.454. It acted on the freezing point according to the following table, which is constructed in all respects like the preceding.

The greatest difference between the calculation and experiment appears here to be only ⅘ of a degree; but that is more than can well be attributed to inaccuracy. These mixtures cooled remarkably well; that in which the water was to the acid as 7.64 : 1 sunk down to 6° before it froze. The ice heated about a degree before it was melted.

Proportion of water to acid.	Freezing point by experiment.	Freezing point by calculation.
16.8 : 1	26⅕°	25⅖°
10 : 1	22 –	21½
7.64 : 1	18	18
5.06 : 1	10½	11
4.26 : 1	7	7

Spirit of salt being a very weak acid, its increase of ratio was not perceptible within the limits to which he was confined.

Muriatic acid.

Proportion of water to acid.	Freezing point by experiment.	Freezing point by calculation.
10 : 1	25°	25°
5.1 : 1	18⅘	18⅓
3.05 : 1	9⅕	9⅓

This table is constructed as the foregoing; and the specific gravity of the marine acid was 1.163.

Salt of tartar, such as is usually sold in the shops, was the vegetable alkali employed. It did not readily deliquesce, and consequently was not very caustic: the cold it produced with snow was — 12°.

Salt of tartar.

Proportion of water to alkali.	Freezing point by experiment.	Freezing point by calculation.
10 : 1	27¼°	26⅓°
7.5 : 1	25⅓	24⅘
5 : 1	22¼	21⅕
3 : 1	15	14
2.5 : 1	11¾	10⅓
2 : 1	5	5

Here the greatest difference between the calculation and experiment is something more than 1¼°, in sinking the freezing point from 22¼° to 5°; but in the higher freezing points of the table it is less, as well as in the lower. Perhaps this irregularity in the experiments is in part to be ascribed to an impurity in the salt of tartar; a turbid appearance, and at length a deposition took place in all these solutions, but principally in the stronger, occasioned probably by tartar of vitriol, with which that salt is so frequently mixed.

The mineral alkali tried, which was the crystallized soda of the shops, showed no increase of ratio; but the scale of its operation was too small for a proper judgment to be formed.

Mineral alkali.

Proportion of water to alkali.	Freezing point by experiment.	Freezing point by calculation.
10 : 1	30° —	29$\frac{9}{10}$°
5 : 1	27⅘	27⅘

This salt would not remain suspended in much greater proportion in the cooled water. He considered the solutions as decreasing in their freezing point by an equal ratio: possibly, if the salt of tartar had been crystallized, and perfectly saturated with fixed air, it would also have acted in the manner of a neutral salt, and produced no increase upon the ratio.

Volatile alkali.

Proportion of water to alkali.	Freezing point by experiment.	Freezing point by calculation.
10 : 1	25°	25¼°
5.18 : 1	19	19

Dr. B.'s volatile alkali, being the sal volatilis salis ammoniaci, was tried only in 2 proportions. Here also there is no appearance of an increase of ratio, but rather the contrary.

The specific gravity of the spirit of wine employed was .829 at 62°. It depressed the freezing point according to the following table: The total difference between the calculation and experiment, on a reduction of the freezing point from $24\frac{1}{2}$ to 4°, is $\frac{3}{4}$ of a degree; but the intermediate points are very irregular, and is an opposite sense, as if the ratio were decreasing. It seems more probable, that some inaccuracy in the experiments, perhaps owing in part to the evaporation of the spirits, should be the occasion of this, than that there should be such an irregularity in the law.

Spirit of wine.

Proportion of water to spirit.	Freezing point by experiment	Freezing point by calculation.
8.5 : 1	$24\frac{1}{2}$°	$23\frac{3}{4}$°
5 : 1	17	18
3.66 : 1	$12\frac{1}{2}$	$12\frac{3}{4}$
3 : 1	$8\frac{1}{2}$	$8\frac{2}{3}$
2.5 : 1	4	4

If any person should allege, that the difference between the observed and computed freezing points in the foregoing tables is not sufficient to establish the increase of ratio, Dr. B. can only reply, that it appears to him greater than can reasonably be ascribed to error in the experiment; especially as similar experiments with neutral salts, not conducted more attentively, agreed so well together in pointing out a different law. It must be allowed however, that the experiments do not show any increase of ratio, except in the vitriolic and nitrous acids, salt of tartar, and, with more ambiguity, in spirit of wine; from analogy only he suspects it to take place in the other acids, and in the mineral and volatile alkalis provided they are caustic. That a different law from what prevails in the neutral salts should take place with these substances, seems not surprizing, when it is considered how much stronger attraction they show for water, and how much less limited the proportion is in which they will unite to it: for the same reasons, he should not think it extraordinary, if deliquescent salts, combined with water, should be found to observe the same increasing ratio in depressing the freezing point.

Dr. B. concludes this paper with the account of an experiment to determine the effect of salt on the expansion of water by cold. Pure water begins to show this expansion about the temperature of 40°, that is, 8° above its freezing point. He put a solution of common salt, in the proportion of 4.8 parts of water to one of the salt, and consequently whose freezing point was $8\frac{2}{3}$°, into an apparatus he had used for other experiments of the same kind; and found that the solution continued to contract till it was cooled to 17°, but had sensibly expanded by the time it was cooled to 15. Suppose the expansion to have begun at $16\frac{2}{3}$°, it would be just 8° above its new freezing point. Hence we have reason to conclude, as far as one experiment goes, that the combination of a salt with water has no other effect on its quality of expanding by cold, than to depress the point at which that quality begins to be sensible, just as much as it depresses the point of congelation.

XIX. Additional Experiments and Observations relating to the Principle of Acidity, the Decomposition of Water, and Phlogiston. By Joseph Priestley, LL.D., F.R.S. p. 313.

When Dr. P. wrote the former paper on this subject, he says he had found that the decomposition of dephlogisticated and inflammable air, by means of the electric spark, produced an acid liquor, which Dr. Withering found to be the nitrous; though it should have been observed, that he expressed some doubt whether the liquor did not also contain some other acid besides the nitrous. Dr. P. has since that time been desirous to ascertain the quantity of acid producible from a given quantity of air; and with this view he gave Mr. Keir as much of the liquor as he had collected from the decomposition of about 500 ounce measures of dephlogisticated air, and the usual proportion of inflammable air mixed with it. The liquor, he informed Dr. P., was 442 grains, of the specific gravity of 1022, that of water being 1000, and that it contained as much acid as was equivalent to 12.54 grains of concentrated acid of vitriol; which quantity of vitriolic acid is capable of saturating as much vegetable fixed alkali as is contained in 22¼ grains of dry nitre, or about 23¼ grains of nitre crystallized in mean temperature. The sediment of the same liquor he also supposed to contain, at least, as much acid as the liquor itself. From the preceding data, given by Mr. Keir, and making allowance for the indefinite quantity of water contained in the concentrated acid of vitriol, Dr. P. thinks that not much more than a 20th part of dephlogisticated air is the acidifying principle, and that 19 parts are water.

Though Mr. Keir found the greatest part of the acid in the liquor with which Dr. P. furnished him to be the nitrous, there were evident signs of its containing a small portion of marine acid, by its making a precipitation with a solution of silver in nitrous acid. But this mixture of marine acid, he observes, is constantly found to accompany the production of nitre in the operations of nature. Whether the different substances from which the dephlogisticated air was extracted made any difference in this case, Dr. P. cannot tell; but that which he gave Dr. Withering was from minium, and that which Mr. Keir examined was from manganese. In the notes which Dr. P. took of the first production of this liquor he termed it blue, and Dr. Withering also calls it blue, and once a greenish blue; but that which he gave Mr. Keir, and all that he got afterwards, was a decided and deep green, which Mr. Keir thinks to be owing to the phlogistication of the nitrous acid.

That water enters into the constitution of every kind of air Dr. P. supposed, because it certainly does into that of inflammable, fixed, and dephlogisticated air, and because none of them can be produced except by processes in which water either certainly is, or may be well supposed to be present. That nitrous air also

contains water, he found from the iron that is heated in it becoming a proper finery cinder. At the publication of his last volume of experiments, he had found, that iron heated in nitrous air acquired weight, and that what remained of the air was phlogisticated air. Having since that time repeated this experiment, and afterwards heated the iron, which was by this means increased in weight, in inflammable air, the iron lost its additional weight, and water was copiously produced, as in the same process with finery cinder, or, as he sometimes calls it, scale of iron. As nitrous air may be deprived of its water, and become phlogisticated air by heating iron in it, he finds that it undergoes the same change by being repeatedly transmitted through hot porous earthen tubes, through which he had discovered that vapour will pass one way, while the air contiguous to the heated tube will pass the other.

That nitrous air contains water, and that this water can contribute to the formation of fixed air, is evident from the following experiment. He heated 5 grains of charcoal of copper in 8 ounce-measures of nitrous air, till it was increased to 10 ounce-measures, and the charcoal had lost 1 grain. Examining the air, he found about $\frac{1}{4}$ of it to be fixed air, and the remainder phlogisticated. It seems therefore, that nitrous air consists of water, and something that may be called the basis of nitrous acid, or that substance which, when united to dephlogisticated air, will make nitrous acid; and this seems to be pure phlogiston, since it is found, as the preceding experiments show, in the purest inflammable air. May we not hence infer, that the nitrous is the simplest of all the acids, and perhaps the basis of all the rest?

Mr. Watt desires it might be mentioned as his conjecture, that the nitrous acid is contained in the inflammable air as the acid of vitriol is in sulphur, the phosphoric in phosphorus, &c.; and that the dephlogisticated air does nothing more than develope the acid. Mr. Keir, who was led to expect that an acid must be the result of the union of dephlogisticated and inflammable air, because some acid is always the consequence of its union with other inflammable substances, thinks that both may be necessary ingredients in it. Further experiments may throw more light on the subject.

To the analysis of the above acid liquor, by Dr. Withering, he adds the following inferences: these, Sir, are all the trials I have made with the liquors produced in your experiment. They pretty clearly prove the acid generated to be the same, whether the dephlogisticated air was procured from red precipitate of mercury, from minium, or from manganese, and that this acid is the nitrous acid. It is not quite so clear why the liquor and sediment in § 4 gave no stronger marks of the presence of nitrous acid; but it is evident that the acid had united itself to the iron, if not to the tin, of the vessel employed: and I find, that when nitrous acid is fully saturated with iron by being boiled with it, and fixed

alkali is added, this mixture submitted to distillation, with the addition of concentrated vitriolic acid, yields no red vapours, and very little smell of nitrous acid.

And to Mr. Keir's letter on the analysis of the same acid, he adds, if, on examining the acids which you or others may hereafter obtain by the inflammation of airs, a mixture of marine acid be constantly found to accompany the production of the nitrous, the fact will be only analogous to all the other known productions of nitrous acid; in all which, either in the natural formation of nitre as in Spain and India, or in the nitre beds and walls made by art, a very large proportion of marine salts is constantly observed to accompany the nitre. Other particulars on this subject may be found by consulting the pamphlet in which this paper was separately printed by Dr. Priestley.

XX. On the Probabilities of Survivorships between Two Persons of any Given Ages, and the Method of Determining the Values of Reversions Depending on those Survivorships. By Mr. Wm. Morgan. p. 331.

The hypothesis of an equal decrement of life, adopted by M. de Moivre, for the purpose of facilitating the computations of life annuities, has not only been rendered unnecessary by the late publication of many excellent tables deduced from real observations, but has also been found so very incorrect in some cases, that probably little or no recourse will ever be had to it in future. But though the direct application of this hypothesis may be laid aside, there is danger of its not being entirely abandoned; and mathematicians may still be led to reason from this principle, by deriving their rules from the expectations rather than from the real probabilities of life. The ingenious Mr. Thomas Simpson has contented himself with this inaccurate method in his select exercises, and he has been followed in it by most other writers on the subject. Even in those cases which involve only 2 lives, the errors are often considerable, especially if the expectations are derived from the London, the Sweden, or any other tables in which the decrements of life are unequal. But when 3 lives are involved in the question, these errors are generally enormous; nor is it ever safe, when the ages of those lives differ very much, to have recourse to rules which are founded on this principle. The 3 following problems, though the most common in the doctrine of survivorships, have never hitherto been solved in a manner strictly true. The 2d of them is of particular importance, and I have taken much pains to examine how far Mr. Simpson's solution of it may be depended on. It has indeed been solved by M. de Moivre, and Mr. Dodson: but the first of these writers has erred most egregiously in the solution itself, and the other having derived his rule from a wrong hypothesis, has rendered it of no use. It is much to be wished that the solutions of all cases in reversions and survivorships were

deduced, like the 3 following, from the real probabilities of life. Most of those which are now in use are at best but approximations, and can never be relied on with any tolerable degree of satisfaction.

PROB. I.—Supposing the ages of 2 persons, A and B, to be given; to determine the probabilities of survivorship between them, from any table of observations.

Solution.—Let a represent the number of persons living in the table at the age of A the younger of the 2 lives. Let a', a'', a''', a'''', &c. represent the decrements of life at the end of the 1st, 2d, 3d, 4th, &c. years from the age of A. Let b represent the number of persons living at the age of B the older of the 2 lives, and c, d, e, f, &c. the number of persons living at the end of the 1st, 2d, 3d, 4th, &c. years from the age of B. Supposing now it were required to determine the probability of B's surviving A in the first year. It is manifest that this event may take place either by A's dying before the end of the year and B's surviving that period, or by the extinction of both the lives, restrained however to the contingency of B's having died last. The probability that A dies in the first year, and that B survives it, is expressed by the fraction $\frac{a'c}{ab}$. The probability that both the lives die in this year is expressed by the fraction $\frac{a'(b-c)}{ab}$; and as it is very nearly an equal chance that A dies first, this fraction should be reduced one-half, and then it will become $= \frac{a'(b-c)}{2ab}$. Hence the whole probability of B's surviving A in the first year will be $= \frac{a'c}{ab} + \frac{a'(b-c)}{2ab} = \frac{a'(b+c)}{2ab}$. In like manner, the probability of B's surviving A in the 2d, 3d, 4th, &c. years may be found $= \frac{a''(c+d)}{2ab} \ldots \frac{a'''(d+e)}{2ab} \ldots \frac{a''''(e+f)}{2ab}$, &c. respectively; therefore the whole probability of B's surviving A will be $= \frac{1}{ab} \times (\frac{b+c}{2}a' + \frac{c+d}{2}a'' + \frac{d+e}{2}a''' + \frac{e+f}{2}a''''$, &c.) Having found, by the preceding series, the probability of B, the elder, surviving A the younger; the other expression, which denotes the probability of A's surviving B, is well known to be the difference between the foregoing series and unity.

The sum of this series might easily be determined from tables of the expectations of single and joint lives. But no such table as the latter having ever been computed, it will by no means be found a laborious undertaking to compute a table of the probabilities of survivorship between 2 persons of all ages immediately from this series, without having recourse to the expectations of life. For if the probability of survivorship between any 2 persons be found, the probability between 2 persons 1 year younger is obtained with little difficulty, and by proceeding in this manner a whole table may be formed in less time than

would be necessary for computing one of the expectations of 2 joint lives. To exemplify this, I shall just set down a few operations for determining the probability of survivorship, according to the Northampton Table of Observations, between 2 persons whose common difference of age is 10 years.

Age of B.	Age of A.	Probability of B's surviving A.	Probability of A's surviving B.
96	86	$\frac{1}{1 \times 145} \times (\frac{0 + 1}{2} \times 34) = 1173$	$1 - .1173 = .8887$
95	85	$\frac{1}{4 \times 186} \times (\frac{4 + 1}{2} \times 41 + 17) = .1606$..	$1 - .1606 = .8394$
94	84	$\frac{1}{9 \times 234} \times (\frac{9 + 4}{2} \times 48 + 119.5) = .2049$	$1 - .2049 = .7951$
93	83	$\frac{1}{16 \times 289} \times (\frac{16 + 9}{2} \times 55 + 431.5) = .2420$	$1 - .2420 = .7580$
92	82	$\frac{1}{24 \times 346} \times (\frac{24 + 16}{2} \times 57 + 1119) = .2720$	$1 - .2720 = .7280$
91	81	$\frac{1}{34 \times 406} \times (\frac{34 + 24}{2} \times 60 + 2259) = .2897$	$1 - .2897 = .7103$
90	80	$\frac{1}{46 \times 469} \times (\frac{46 + 34}{2} \times 63 + 3999) = .3022$	$1 - .3022 = .6978$

It may easily be seen, from these specimens, in what manner the probabilities of survivorship between 2 younger lives are deduced from the probabilities between 2 older lives, provided their common difference of age be the same; for the numbers 17 .. 119.5 ... 431.5, &c. in the 2d, 3d, 4th, &c. series, are the sums of the series next preceding. Thus 17 is $= 34 \times \frac{1}{2}$ ... 119.5 is $= 41 \times \frac{5}{2} + 17$ .... 431.5 is $= 48 \times \frac{13}{2} + 119.5$, &c. It may be necessary to observe further, that if the ages of the 2 persons be equal, the probability of survivorship between them being likewise equal, is expressed by the fraction $\frac{1}{2}$; and that this affords an instance of the accuracy of the foregoing investigation; for the series expressing the probability in this case is the same with this fraction, the chance of survivorship becoming then (since $a = b$; $a' = b - c$; $a'' = c - d$, &c.; and $(b + c) \times a' = (b + c) \times (b - c) = b^2 - c^2$, &c.) $= \frac{bb - cc}{2bb} + \frac{cc - dd}{2bb}$, &c. $= \frac{1}{2}$.

Mr. Simpson, in his Treatise on Annuities and Reversions, (Lemma 2, p. 100,) has given a curve whose area determines the probability of survivorship between 2 persons according to any table of observations. If one of the lives be not very young, so that the equidistant ordinates may not be too few, this area is sufficiently correct. But if the elder of the 2 lives is under 20 years of age, it becomes necessary to assume so many equidistant ordinates to render the solution accurate, when the decrements of life are unequal, that the operation is rendered much too laborious for use; nor do I know that it can be necessary to

have recourse to this area in any case, especially as the true probabilities of survivorship are so easily computed from the preceding series.

The following table has been formed in the manner described above; and as no such table has ever been attempted before, I have been the more desirous to render it complete, by computing the probabilities of survivorship between 2 persons of all ages, whose common difference is not less than 10 years. Instead also of supposing certainty to be denoted by unity, I have assumed 100 for this purpose; so that the sums in the adjoining columns express the number of chances in 100 which are in favour of B or A's surviving the other.

Table, showing the Probabilities of Survivorship between 2 Persons of all Ages, whose Common Difference of Age is not less than 10 Years, computed from the Northampton Table of Observations in Dr. Price's Treatise on Reversionary Payments.

Ten Years Difference.

Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
11.. 1	58.59	41.41	33.. 23	41.97	58.03	55.. 45	38.53	61.47	77.. 67	31.20	68.80
12.. 2	51.36	48.64	34.. 24	41.83	58.17	56.. 46	38.35	61.65	78.. 68	30.90	69.10
13.. 3	48.23	51.77	35.. 25	41.70	58.30	57.. 47	38.16	61.84	79.. 69	30.47	69.53
14.. 4	45.98	54.02	36.. 26	41.56	58.44	58.. 48	37.97	62.03	80.. 70	30.00	70.00
15.. 5	44.70	55 30	37.. 27	41.41	58 59	59.. 49	37.77	62.23	81.. 71	29.58	70.42
16.. 6	43.43	56.57	38.. 28	41.26	58.74	60.. 50	37.56	62.44	82.. 72	29 19	70.81
17.. 7	42.53	57.47	39.. 29	41.10	58.90	61.. 51	37.30	62.70	83.. 73	28.90	71.10
18.. 8	41.91	58.09	40.. 30	40.94	59.06	62.. 52	37.00	63.00	84.. 74	29.02	70.98
19.. 9	41.60	58.40	41.. 31	40.78	59.22	63.. 53	36.68	63.32	85.. 75	29.17	70.83
20.. 10	41.53	58.47	42.. 32	40.63	59.37	64.. 54	36.34	63.66	86.. 76	29.25	70.75
21.. 11	41.58	58.42	43.. 33	40.49	59.51	65.. 55	35.97	64.03	87.. 77	29.42	70.58
22.. 12	41.68	58.32	44.. 34	40.34	59.66	66.. 56	35.58	64.42	88.. 78	29.94	70.06
23.. 13	41.78	58.22	45.. 35	40.19	59.81	67.. 57	35.17	64.83	89.. 79	30.29	69.71
24.. 14	41.90	58.10	46.. 36	40.04	59.96	68.. 58	34.74	65.26	90.. 80	30.22	69 78
25.. 15	42.02	57.98	47.. 37	39.88	60.12	69.. 59	34.30	65.70	91.. 81	28.97	71.03
26.. 16	42.16	57.84	48.. 38	39.71	60.29	70.. 60	33.85	66.15	92.. 82	27.20	72.80
27.. 17	42.26	57.74	49.. 39	39.54	60.46	71.. 61	33.38	66.62	93.. 83	24.20	75.80
28.. 18	42.32	57.68	50.. 40	39.38	60.62	72.. 62	32.90	67.10	94.. 84	20.49	79.51
29.. 19	42.34	57.66	51.. 41	39.23	60.77	73.. 63	32.46	67.54	95.. 85	16.06	83.94
30.. 20	42.31	57.69	52.. 42	39.07	60.93	74 . 64	32.04	67.96	96.. 86	11.73	88.27
31.. 21	42.22	57.78	53.. 43	38.90	61,10	75.. 65	31.70	68.30			
32.. 22	42.09	57.91	54.. 44	38.81	61.19	76.. 66	31.45	68.55			

Twenty Years Difference.

21.. 1	52.46	47.54	32.. 12	34.42	65.58	43.. 23	33.59	66.41	54.. 34	30.60	69.40
22.. 2	44.33	55.67	33.. 13	34.44	65.56	44.. 24	33.35	66.65	55.. 35	30.32	69.68
23.. 3	40.88	59.12	34.. 14	34.47	65.53	45.. 25	33.09	66.91	56.. 36	30.03	69.97
24.. 4	39.00	61.00	35.. 15	34.51	65.49	46.. 26	32.83	67.17	57.. 37	29.73	70.27
25.. 5	37.69	62.31	36.. 16	34.56	65.44	47.. 27	32.56	67.44	58.. 38	29.43	70.57
26.. 6	36.40	63 60	37.. 17	34.58	65.42	48.. 28	32.28	67.72	59.. 39	29.13	70.87
27.. 7	35.48	64.52	38.. 18	34.54	65.46	49.. 29	31.99	68.01	60.. 40	28.82	71.18
28.. 8	34.84	65.16	39.. 19	34.44	65.56	50.. 30	31.70	68.30	61.. 41	28.49	71.51
29.. 9	34.52	65.48	40.. 20	34.29	65.71	51.. 31	31.43	68.57	62.. 42	28.14	71.86
30.. 10	34.41	65.59	41.. 21	34.08	65.92	52.. 32	31.16	68.84	63.. 43	27.74	72.26
31.. 11	34.40	65.60	42.. 22	33.84	66.16	53.. 33	30.88	69.12	64.. 44	27.33	72.67

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Twenty Years Difference.

Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
65..45	26.90	73.10	73..53	22.91	77.09	81..61	18.23	81.77	89..69	16.23	83.77
66..46	26.46	73.54	74..54	22.35	77.65	82..62	17.57	82.43	90..70	15.67	84.33
67..47	26.01	73.99	75..55	21.81	78.19	83..63	17.03	82.97	91..71	14.50	85.50
68..48	25.55	74.45	76..56	21.31	78.69	84..64	16.73	83 27	92 .72	13.05	86.95
69..49	25.09	74.91	77..57	20.80	79.20	85..65	16.55	83.45	93 .73	11.08	88.92
70..50	24.61	75.39	78..58	20.24	79.76	86..66	16.29	83.71	94..74	9.24	90.76
71..51	24.06	75.94	79..59	19.59	80.41	87..67	16.38	83.62	95..75	7.17	92.83
72..52	23.49	76.51	80..60	18.90	81.10	88..68	16.44	83.56	96..76	5.12	94.88

Thirty Years Difference.

31.. 1	48.23	51.77	48..18	26.90	73.10	65..35	20.93	79.07	82..52	12.46	87.54
32.. 2	39.36	60.64	49..19	26.72	73.28	66..36	20.47	79.53	83..53	11.94	88.06
33.. 3	35.57	64.43	50..20	26.50	73.50	67..37	20.01	79.99	84..54	11.60	88.40
34.. 4	32.86	67.14	51..21	26.21	73 79	68..38	19.54	80.46	85..55	11.30	88.70
35.. 5	31.35	68.65	52..22	25.89	74.11	69..39	19.06	80.94	86..56	11.04	88.96
36.. 6	29.85	70.15	53..23	25.56	74.44	70..40	18.59	81.41	87..57	10.80	89.20
37.. 7	28.83	71.17	54..24	25.22	74.78	71..41	18.10	81.90	88..58	10.63	89.37
38.. 8	27.98	72.02	55..25	24.87	75.13	72..42	17.58	82.42	89..59	10.25	89.75
39.. 9	27.53	72.47	56..26	24.52	75.48	73..43	17.05	82.95	90..60	9.65	90.35
40..10	27.33	72.67	57..27	24.16	75.84	74..44	16.53	83.47	91..61	8.68	91.32
41..11	27.24	72.70	58..28	23.79	76.21	75..45	16.04	83.96	92..62	7.54	92.46
42..12	27.18	72.82	59..29	23.41	76.59	76..46	15.60	84.40	93..63	6.18	93.82
43..13	27.14	72.86	60 .30	23.02	76.98	77..47	15.51	84.85	94..64	4.93	95.07
44..14	27.11	72.89	61..31	22.63	77.37	78..48	14.68	85.32	95..65	3.68	96.32
45..15	27.08	72.92	62..32	22.23	77.77	79..49	14.16	85.84	96..66	2.58	97.42
46..16	27.06	72.94	63..33	21.81	78.19	80..50	13.61	86.39			
47..17	27.01	72.99	64..34	21.38	78.62	81..51	13.04	86.96			

Forty Years Difference.

41.. 1	43.57	56.4	55..15	20.11	79.89	69..29	15.32	84.68	83..43	8.76	91 24
42.. 2	33.98	66.02	56..16	20.05	79.95	70..30	14.83	85.17	84..44	8.46	91.54
43.. 3	29.85	70.15	57..17	19.96	80.04	71..31	14.34	85.66	85..45	8.18	91.82
44.. 4	26.88	73.12	58..18	19.81	80.19	72..32	13.84	86.16	86..46	7.95	92.05
45.. 5	25.20	74.80	59..19	19.59	80.41	73..33	13.35	86.65	87..47	7.74	92.26
46.. 6	23.53	76.47	60..20	19.31	80.69	74..34	12.87	87.13	88..48	7.59	92.41
47.. 7	22.30	77.70	61..21	18.96	81.04	75..35	12.42	87.58	89..49	7.32	92.68
48.. 8	21.40	78.60	62..22	18.55	81.45	76..36	11.99	88.01	90..50	6.90	93.10
49.. 9	20.86	79.14	63..23	18.12	81.88	77..37	11.56	88.44	91..51	6.17	93.83
50..10	20.59	79.41	64..24	17.68	82.32	78..38	11.12	88.88	92..52	5.33	94.67
51..11	20.45	79.55	65..25	17.22	82.78	79..39	10.64	89.36	93..53	4.32	95.68
52..12	20.35	79.65	66..26	16.76	83.24	80..40	10 21	89.79	94 .54	3.42	96.58
53..13	20.26	79.74	67..27	16.29	83.71	81..41	9.68	90.32	95..55	2.51	97.49
54..14	20.18	79.82	68..28	15.81	84.19	82..42	9.19	90.81	96..56	1.73	98.27

Fifty Years Difference,

51.. 1	39.16	60.84	63..13	13.90	86.10	75..25	10.32	89.68	87..37	5.91	94.09
52.. 2	28.87	71.13	64..14	13.76	86.24	76..26	9.91	90.09	88..38	5.74	94.26
53.. 3	24.46	75.54	65..15	13.62	86.38	77..27	9.50	90.50	89..39	5.48	94.52
54.. 4	21.28	78.72	66..16	13.50	86.50	78..28	9.07	90.93	90..40	5.11	94.89
55.. 5	19.48	80.52	67..17	13.35	86.65	79..29	8.61	91.39	91..41	4.55	95.45
56.. 6	17.68	82.32	68..18	13.14	86.86	80..30	8.15	91.85	92..42	3.92	96.08
57.. 7	16.35	83.65	69..19	12.86	87.14	81..31	7.70	92.30	93..43	3.15	96.85
58.. 8	15.35	84.65	70..20	12.53	87.47	82..32	7.27	92.73	94..44	2.47	97.53
59.. 9	14.74	85.26	71..21	12.11	87.89	83..33	6.88	93.12	95..45	1.80	98.20
60..10	14.42	85.58	72..22	11.65	88.35	84..34	6.60	93.40	96..46	1.23	98.77
61..11	14.22	85.78	73..23	11.20	88.80	85..35	6.35	93.65			
62..12	14.06	85.94	74..24	10.75	89.25	86..36	6.12	93.88			

Sixty Years Difference.

Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
61.. 1	34.94	65.06	70.. 10	8.73	91.27	79.. 19	7.19	92.81	88.. 28	4.72	95.26
62.. 2	24.13	75.87	71.. 11	8.46	91.54	80.. 20	6.90	93.10	89.. 29	4.48	95.52
63.. 3	19.52	80.48	72.. 12	8.25	91.75	81.. 21	6.55	93.45	90.. 30	4.13	95.87
64.. 4	16.19	83.81	73.. 13	8.05	91.95	82.. 22	6.16	93.84	91.. 31	3.63	96.37
65.. 5	14.28	85.72	74.. 14	7.88	92.12	83.. 23	5.81	94.19	92.. 32	3.10	96.90
66.. 6	12.37	87.63	75.. 15	7.75	92.25	84.. 24	5.56	94.44	93.. 33	2.48	97.52
67.. 7	10.92	89.08	76.. 16	7.68	92.32	85.. 25	5.32	94.68	94.. 34	1.94	98.06
68.. 8	9.82	90.18	77.. 17	7.59	92.41	86.. 26	5.11	94.89	95.. 35	1.40	98.60
69.. 9	9.12	90.88	78.. 18	7.43	92.57	87.. 27	4.90	95.10	96.. 36	0.95	99.05

Seventy Years Difference.

71.. 1	30.49	69.51	78.. 8	5.53	94.47	85.. 15	3.68	96.32	92.. 22	2.63	97.37
72.. 2	19.45	80.55	79.. 9	4.84	95.16	86.. 16	3.70	96.30	93.. 23	2.10	97.90
73.. 3	14.87	85.13	80.. 10	4.44	95.56	87.. 17	3.73	96.27	94.. 24	1.64	98.36
74.. 4	11.59	88.41	81.. 11	4.18	95.82	88.. 18	3.75	96.23	95.. 25	1.18	98.82
75.. 5	9.80	90.20	82.. 12	3.97	96.03	89.. 19	3.67	96.33	96.. 26	0.80	99.20
76.. 6	7.98	92.02	83.. 13	3.80	96.20	90.. 20	3.45	96.55			
77.. 7	6.60	93.40	84.. 14	3.72	96.28	91.. 21	3.09	96.91			

Eighty Years Difference.

81.. 1	24.95	75.05	85.. 5	6.34	93.66	89.. 9	2.50	97.50	93.. 13	1.25	98.75
82.. 2	14.37	85.63	86.. 6	4.92	95.08	90.. 10	2.16	97.84	94.. 14	0.97	99.03
83.. 3	10.34	89.66	87.. 7	3.85	96.15	91.. 11	1.86	98.14	95.. 15	0.70	99.30
84.. 4	7.63	92.37	88.. 8	3.04	96.96	92.. 12	1.57	98.43	96.. 16	0.49	99.51

Ninety Years Difference.

Ages.	Probabilities.		Ages.	Probabilities.	
91.. 1	18.93	81.07	94.. 4	3.12	96.88
92.. 2	9.18	90.82	95.. 5	2.12	97.88
93.. 3	5.53	94.46	96.. 6	1.13	98.58

PROB. 2. Supposing the ages of A and B to be given; to determine, from any table of observations, the present value of the sum S payable on the contingency of one life's surviving the other.

Solution.—Let r denote 1*l.* increased by its interest for a year, and let all the other symbols be the same as in the preceding problem. Let the life of B also be supposed to be the older of the 2 lives; and then it will follow, by reasoning as in the solution of that problem, that the present value of S to be received on the death of A, should that happen in the life time of B, will be expressed by the series $s \times (\frac{b+c}{2abr}a' + \frac{c+d}{2abr^2}a'' + \frac{e+e}{2abr^3}a''' + \frac{c+f}{2abr^4}a''''$ &c.) This series may be resolved into the 2 following; $\frac{s}{2} \times (\frac{ca'}{abr} + \frac{da''}{abr^2} + \frac{ea'''}{abr^3} + \frac{fa''''}{abr^4}$ &c.) $+ \frac{s}{2} \times (\frac{ba'}{abr} + \frac{ca''}{abr^2} + \frac{da'''}{abr^3} + \frac{fa''''}{abr^3}$ &c.) The first of these 2 series may be again resolved into $\frac{s}{2} \times (\frac{c}{br} - \frac{ca-ca'}{abr} + \frac{d}{br^2} - \frac{da-da'-da''}{abr^2} + \frac{e}{br^3} - \frac{ea-ea'-ea''-ea'''}{abr^3}$ &c.) —

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$\text{s} \times \frac{c}{2br} \times (\frac{d}{cr} - \frac{da - da'}{acr} + \frac{e}{cr^2} - \frac{ea - ea' - ea''}{acr^3}$ &c.) Let B denote the value of an annuity on the life of B, C the value of an annuity on a life 1 year older than B, AB and AC the values of annuities on the joint lives of A and B and of A and C, and these series will be $= \frac{\text{s} \times (\text{B} - \text{AB})}{2} - \frac{\text{s} \times c \times (\text{C} - \text{AC})}{2br}$. Again, the 2d series above mentioned, or $\frac{\text{s}}{2} \times (\frac{ba'}{abr} + \frac{ca''}{abr^2} + \frac{da'''}{abr^3}$ &c.), by pursuing the same steps, may be found $= \frac{\beta \times \text{s}}{2b} \times (\text{K} - \text{AK}) - \frac{\text{s}\,(\text{B} - \text{AB})}{2r}$, where β denotes the number of persons living at the age of a person 1 year younger than B, K the value of an annuity on that life, and AK the value of an annuity on the joint lives of A and K. The whole value of the survivorship is therefore $= \text{s} \times (\frac{r - 1) \cdot (\text{B} - \text{AB})}{2r} + \frac{\beta \cdot (\text{K} - \text{AK})}{2b} - \frac{c \cdot (\text{A} - \text{AC})}{2br}$. Q. E. D.

Having now the value of the sum s payable on the contingency of B's surviving A, the value of the same sum, payable on the contingency of A's surviving B, is easily obtained by the well known method of subtracting the value found above from the whole value of the reversion after the extinction of the joint lives of A and B. It is evident that the exactness of the above rule must depend on the accuracy with which the values of the single and joint lives are computed. Being possessed of such tables for all ages, even with respect to the joint lives, I have computed the following values, that it may be seen how far Mr. Simpson's approximation*, the only rule now in use, may be depended on.

Age of B.	Age of A.	† Value of 100l. payable on the death of A if B survives him.		Age of B.	Age of A.	Value of 100l. payable on the death of A if B survives him.		Age of B.	Age of A.	Value of 100l. payable on the death of A if B survives him.	
		True value.	Simpson's value.			True value	Simpson's value.			True value.	Simpson's value.
10	2	32.67	26 05	40	10	17.10	19.07	60	60	38.88	38.92
10	10	24.74	24.75	40	40	32.86	32.87	70	2	18.36	9.81
20	2	29.99	24.73	50	2	23.36	17.06	70	10	7.07	9.15
20	10	22.11	23.50	50	10	14.10	16.21	70	40	15.35	15.78
20	20	27.95	27.96	50	20	18.65	19.29	70	70	42.34	42.33
30	2	28.79	22.60	50	50	35.80	35.85	80	2	14.46	5.71
30	10	19.84	21.47	60	2	21.52	13.61	80	10	3.93	5.43
30	30	30.22	30.21	60	10	10.65	12.93	80	50	12.05	12 00
40	2	26.65	20.07	60	30	17.51	18.19	80	80	45.47	45.45

* It must be remembered, that the correction explained by Dr. Price, in vol. i. p. 39, &c. of his Treatise on Reversionary Payments, must be applied to Mr. Simpson's rule; that is, when the reversion is a sum and not an estate, the value found by the rule must be divided by 1l. increased by its interest for a year.

† These values have been computed at 3 per cent. and from the Northampton Table of Observations.

From this table it appears that Mr. Simpson's approximation in the middle stages of life is sufficiently accurate; but that it is exceedingly defective when the life of A is very young. It should also be remembered, that these values have been computed at a low rate of interest, and from the Northampton Table of Observations, in which the decrements of life come nearer to M. de Moivre's hypothesis than in any other table. But if the computations be made at a higher rate of interest even from this table, the approximation does not always agree so well, as will appear from the following specimens calculated at 5 per cent.

Age of B.	Age of A.	Value of 100*l.* payable on the death of A if B survives him, by the Northampton table at 5 per cent.	
		True value.	Simpson's value.
20	2	25.09	18 46
20	10	15.49	17.54
40	2	23.57	1[illegible]97
60	2	19.83	1 75
60	40	18.73	19.61

Value of 100*l.* payable on the death of A if B survives him, according to the Sweden Table of Observations, and at 4 per cent.							
Age of B.	Age of A.	True value.	Simpson's value.	Age of B.	Age of A.	True value.	Simpson's value.
20	2	21.47	16.80	60	36	12 29	16.81
20	14	15 42	17.82	60	42	16.11	19.58
20	20	20.01	19.84	60	60	36.96	36.34
40	4	23.53	14.22	76	40	9.21	9.81
40	16	13.71	16.23	76	52	12.58	14.00
40	28	17.60	20.44	76	64	23.81	22.81
40	40	27.62	27.00	76	76	42.90	43.29
60	24	9.39	13.01				

In order further to compare Mr. Simpson's approximation with the true value, I have inserted in the foregoing table a few computations deduced from the Sweden Table of Observations, in which the decrements of life are unequal. From these instances the approximation appears to be more defective in proportion as the probabilities of life differ from the hypothesis.

PROB. 3.—The ages of A and B being given; to determine the value of the sum s, payable on the extinction of one life in particular, should that happen after the extinction of the other life.

Solution.—Supposing B to be the older of the 2 lives, and the sum s to become payable on his decease; it is evident that this payment at the end of the first year must depend on the contingency of both lives being extinct before this period and of B's dying last. Retaining the same symbols, and reasoning as in the solution of the first problem, this value will be expressed by the fraction $\frac{s \,.\, a' \,.\, (b-c)}{2abr}$. The payment of the sum s at the end of the 2d year will depend on either of 2 events happening. First, that A and B both die in the 2d year after having survived the first, restrained, as above, to the contingency of B's having died last; 2dly, that B dies in the 2d year and A in the 1st year. The value

therefore of s for this year will be expressed by the 2 fractions $\frac{s \cdot a'' \cdot (c-d)}{2abr^2} + \frac{s \cdot (c-d) \cdot a'}{abr^2}$. Again, the payment of s in the 3d year will depend either on A and B's both dying in that year, and B having died last; or on B's dying in that year, and A's dying in the 1st or 2d years. The value therefore of s for this year will be $= \frac{s \cdot a''' \cdot (a-e)}{2abr^3} + \frac{s \cdot (d-e) \cdot (a' + a'')}{abr^3}$. By proceeding in this manner for the other years the whole value of the reversion will be found $= \frac{s}{2} \times \frac{a' \cdot (b-c)}{abr} + \frac{a'' \cdot (c-d)}{abr^2} + \frac{a''' \cdot (d-e)}{abr^3} + \frac{a'''' \cdot (e-f)}{abr^4} + \text{\&c.}) + s \times (\frac{a' \cdot (c-d)}{abr^2} + \frac{(a'+a'') \cdot (d-e)}{abr^3} + \frac{(a'+a''+a''') \cdot (e-f)}{abr^4} + \text{\&c.}$ The first of these series, by proceeding in the same manner as in the solution of the 2d problem, may be found $= \frac{\beta}{2b} \times (K - AK) - \frac{B - AB}{2r} - \frac{1}{2} \cdot (B - AB) + \frac{c}{2br} \times (C - AC)$; and the 2d series may be found $= -\frac{c}{br} \times (C - AC) + \frac{B - AB}{r}$. Hence the whole value of the reversion will be $= s \times (\frac{\beta r \cdot (K - AK) - c \cdot (C - AC)}{2br} - \frac{(r-1 \cdot (B - AB)}{2r}$. Q. E. D.

Having now the value of the sum s depending on the older of the 2 lives dying last, the value of the same sum depending on the younger of the 2 lives dying last is easily obtained, by subtracting the value first found from the whole value of the reversion after the extinction of both lives. The answers computed by this rule differ rather more from those computed by Mr. Simpson's approximation than they do in the preceding problem.

XXI. *Of a Remarkable Transposition of the Viscera. By Matthew Baillie, M. D.* p. 350.

Nothing tends more to illustrate the powers and the wisdom of nature than the investigation of the structure of animals. We there find a most wonderful delicacy of mechanism, and exquisitely adapted to a variety of purposes. This however is not to be better seen by following nature in her common track than by observing her wanderings. In these she often shows more particularly the extent of her powers, and throws light on her ordinary plans. Such circumstances give importance and value to the observation of singular phenomena. The variety in animal structure, an account of which is presented in this account, is a complete transposition in the human subject, of the thoracic and abdominal viscera, to the opposite side from what is natural. It is so extraordinary as scarcely to have been seen by any of the most celebrated anatomists, and indeed has been but very generally noticed at all. The circumstance has been mentioned, but it has not been particularly described so as to make it thoroughly known, or to establish its certainty. It was hanging in the minds of many as doubtful, whether such a

variety did really exist. There is one circumstance that attends the account of the present case, which has not always happened in the record of singular phenomena, viz. that it has been examined by physicians and surgeons of the first reputation in this large town, and has been in some measure open to the gratification of public curiosity.

The person who is the subject of this paper was a male, nearly 40 years of age, somewhat above the middle stature, and of a clean active shape. He was brought for dissection in the common way to Windmill-street. On opening the cavity of the thorax and abdomen, the different situation of the viscera was so striking as immediately to excite the attention of the pupils who were engaged in dissecting it. I began immediately to examine every part of the change with considerable attention: for this purpose, after desiring a drawing to be made of the appearances as they were found on opening the body, I next day injected it.

The mediastinum, or anterior duplicature of the pleura, separating the 2 cavities of the chest from each other, was found to incline obliquely downwards to the right side fully as much as it does commonly to the left side of the chest. The pericardium too inclined obliquely to the right side. On pressing it gently away from the lungs the phrenic nerves came distinctly into view, in their common situation; but the right phrenic nerve ran more obliquely, and was longer than the left. The lung on the right side was divided by a single oblique fissure into 2 lobes, having at the same time a deficiency opposite to the apex of the heart; and the lung on the left side was divided into 3 lobes, exactly contrary to what is found in ordinary cases.

On opening the pericardium the apex of the heart was found to point to the right side nearly opposite to the 6th rib, and its cavities as well as large vessels were completely transposed. What are commonly called the right auricle and ventricle were situated on the left side, and the left auricle and ventricle on the right. The pulmonary artery ascended towards the right side of the chest. The aorta was also directing its arch to the right; and the vena cava superior, as well as inferior, were seen opening into their auricle on the left side of the spine. There was nothing remarkable in the size or general figure of the heart. On the outside of the pericardium the transposition of the larger vessels was very striking. The longer subclavian vein was passing from the left side obliquely to the right before the branches which are sent off from the arch of the aorta. The left carotid and subclavian arteries were found to arise from the arch of the aorta by one common trunk; the right carotid and subclavian separately.

In the duplicature of the pleura behind, or what may be called the posterior mediastinum, there was a change corresponding to what we have already described. The descending aorta was found passing on the right side of the

spine. The œsophagus was before it, inclining more and more to the right towards its lower extremity, and it at length perforated the diaphragm somewhat on the right side of the spine.* The thoracic duct was seen in the middle between the descending aorta and vena azygos, in some places forming a plexus of small branches, in another dividing itself into 2 branches, which afterwards re-united in a common trunk, and at length climbing up to terminate in the angle between the jugular and subclavian veins on the right side of the body. The recurrent nerve of the parvagum on the right side passed round the beginning of the descending aorta, and on the left passed round the common trunk of the carotid and subclavian arteries. The large intercostal nerves being exactly under the same circumstances on each side, it was impossible there could be any transposition in them. It appears then from the foregoing description, that every thing admitting of such a change was completely transposed in the thorax.

The liver was situated in the left hypochondriac region, the small lobe being towards the right, and the great lobe in the left side. The ligaments uniting it to the diaphragm corresponded to this change, the right transverse ligament being longer, and the left being shorter, than usual. The suspensory ligament could undergo little change, except being pushed to the left side along with the liver. On pressing upwards the liver, so as to exhibit its posterior and under surface, the gall bladder was seen on the left side preserving its proper relative situation to the great lobe of the liver, and the vessels of the portæ were found on dissection to be transposed corresponding to the change of circumstances. The hepatic artery was found climbing up obliquely from the right towards the left, before the lobulus spigelii, and entered at the portæ into the substance of the liver by two or three branches on the right of the other vessels. The ductus communis cholidochus was on the left of the other vessels, being formed from the ductus hepaticus and ductus cysticus in the common way, and it passed obliquely downwards on the left, to terminate in the duodenum. What was most remarkable, it terminated in the fore part of the duodenum. The vena portarum passed behind the hepatic artery and ductus communis cholidochus, ascending obliquely towards the left side.

The spleen was situated in the right hypochondriac region, adhering to the diaphragm in the common way. There were 3 spleens, nearly of the size of a pullet's egg, found adhering to the larger spleen by short adhesions, besides 2 other still smaller spleens which were involved in the epiploon at the great end of the stomach. The pancreas was found on the right side behind the stomach, running obliquely from the spleen to the curvature of the duodenum, and had its duct entering in common with the ductus communis cholidochus into the

* The vena azygos was on the left side of the spine opening in the common way into the vena cava superior, which we formerly mentioned to be also transposed in its situation.—Orig.

cavity of that intestine. The splenic vessels were passing along the upper edge of the pancreas to the right side, corresponding to the change of situation in the pancreas and spleen.

The stomach was situated on the right side, partly hid by the small lobe of the liver passing to the left, and terminating in the pylorus, rather on the left side of the spine. The duodenum took a most singular course; it first passed to the right side, behind the small end of the stomach; it then turned on itself, towards the left side; it afterwards took its proper sweep to the right side, passing behind the superior mesenteric artery and mesaraica major vein. The mesentery began to be formed on the right side, instead of the left, as in ordinary cases. The ilium terminated in the great intestine on the left side, and there was in it a diverticulum of considerable size, a lusus not unfrequently occurring. The cæcum was situated on the left psoas magnus and iliacus internus muscles. The transverse arch of the colon passed from the left to the right side of the body, and the sigmoid flexure crossed over the right psoas, to get into the cavity of the pelvis. The kidneys had their vessels transposed; the renal capsules had undergone no change, as no variety could be produced by a transposition.

The aorta passed between the crura of the diaphragm into the cavity of the abdomen, and adhered in its course to the spine on the right side of the vena cava inferior. Its branches were directed in their course corresponding to the peculiar situation of the viscera. The splenic and coronary arteries were passing to the right side, and the hepatic artery obliquely to the left. The superior and inferior mesenteric arteries were directed to the right side. There was no change in the spermatic arteries, any transposition in the testicles, if such a thing could take place, not being capable of affecting them. The lumbar arteries could also undergo little change, except that the left lumbar arteries must necessarily, from the peculiar situation of the aorta, be the longest. The vena cava inferior perforated the tendinous portion of the diaphragm, and adhered in its course to the spine on the left side of the aorta.

The right emulgent vein was much longer than usual, passing from the right kidney before the aorta to terminate in the vena cava superior; and the left emulgent much shorter, passing from the left kidney to the vena cava, which was situated on the left side of the spine. The right spermatic vein was found to open into the right emulgent, and the left into the vena cava inferior, about an inch under the left emulgent. The vena portarum was changed from its natural course, passing obliquely upwards to the left side, and its large branches, viz. the vena splenica, mesaraica major and minor, were all directed towards the right side of the spine. There was no change in the intercostal nerve within the cavity of the abdomen; nor does it seem to be capable of being affected by any transposition of parts. We see then, that there was a complete transposition

of the abdominal viscera, each of them preserving its proper relative situation to the others. In the brain, organs of sense, of generation, the muscles, and blood vessels of the extremities, was found nothing remarkable.

The person seems to have used his right hand in preference to his left, as is usually the case, which was readily discovered by the greater bulk and hardness of that hand, as well as the greater fleshiness of the arm. It was not indeed to be expected he should be left handed. The person, while alive, was not conscious of any uncommon situation of his heart; and his brother has his heart pointing to the left side as in ordinary cases. Indeed, there was little reason to expect that we should meet with any thing particular in the account of his life. His health could not be affected by such a change of situation in his viscera; nor could there arise from it any peculiar symptoms of disease. Still less could there be any connection between such a change and his dispositions, or external actions. He might have known that his heart was directed towards the right side; but if we consider how little every person, especially those of the lower class, are attentive to circumstances not very palpable, it was scarcely to be expected he should know of it.

Notwithstanding the general similarity of parts in the same species of animals, there is no reason why nature should not sometimes deviate from her ordinary plans. Accordingly we find there is much variety in animal structure; but this does not commonly affect the animal functions. Under this restriction the variety is so great in the appearances of every part of an animal, that it is almost impossible to examine any 2 animals of the same species without remarking many differences. In the bony compages of an animal we find little variety in the extremities of bones where there is the apparatus of a joint, because a particular shape is best adapted to a particular kind or latitude of motion. In other parts of the bones, where a difference of features is not material, there is great variety, as in the foramina, depressions, ridges, and sutures of bones. The same general rule will apply to variety in muscles. The principal object is a certain insertion near a joint, so as to give a determined direction of motion. With respect to such insertions, there is, comparatively speaking, little variety; but there is a great difference in the bodies and connections of muscles, which have no share in the regulation of the motion.

There is no part of an animal where there is a greater latitude of variety than in the distribution of blood vessels. The reason of it is very obvious. The only object in the distribution of blood vessels is, to carry blood to every part of the body, and bring it back to the heart. The parts of an animal, in order to be supported, must be visited by successive changes of fresh blood; but it surely cannot be an object of importance whether the blood passes by one rout or another. Hence the variety of blood vessels is extremely great. Still however

there is a method in the deviations of nature, so that they may be marked or noted, the same varieties occurring in different animals.

It cannot be at all important to the function of a viscus, whether it be in one mass, or in separate portions. The structure being the same, the same action will take place. Hence we often find the 2 kidneys joined together, forming one mass; and not unfrequently 2 or 3 spleens, besides the common one. Neither can it be important whether a viscus should always be of the same shape, because its functions do not depend on shape, but on structure: we find accordingly, in this particular, much variety.

There are many of the viscera which are connected together in their functions, or by the junction of large blood vessels, in such a way as to require nearly the same relative situation among themselves. This becomes also necessary in order to preserve the general shape of the animal. Accordingly we find, that when any important viscus is changed in its situation, it affects the situation of other viscera, requiring in them a similar change. We saw in the person who is the subject of this paper, that a change in the situation of the heart and liver was accompanied with a change of situation in the stomach, spleen, pancreas, and in short the whole abdominal viscera. This however is a great deviation in nature; for it is nothing less than changing almost the whole vital system in an animal, and therefore it rarely happens. In such a change it does not appear that the functions can be affected, as they depend on structure and situation, which are both preserved. Hence the person who is the subject of this paper arrived at the age of maturity, and might have continued to live to an extreme old age. The human machine might have been constructed in this way generally, and under such circumstances, what is now called the natural situation of parts would have been as singular as the present phænomenon.

There appears to be less variety in the nervous system of animals of the same species, than in most parts of the body. There is scarcely any difference in the appearance of the brain, and much less in the distribution of the nerves than of the blood vessels. There is also little variety in the organs of sense: perhaps the mechanism in both these is nicer, so that a considerable deviation would interfere with their peculiar functions. The most common great deviations which nature produces in the structure of an animal, are various kinds of monstrosity, by which the animal becomes often unfit for continuing its existence. Why nature should in its greater deviations fall into a very imperfect formation, much below the standard of her common work, does not appear very obvious. It seems that there might have been many varieties where the functions could have been preserved. Perhaps it is with a view to check the propagation of great varieties, so as to preserve a uniformity in the same species of animals.

It has been much agitated, whether monstrosities depend on the original for-

mation, or are produced afterwards in the gradual evolution of an animal. This does not appear to be a question of much importance; nor perhaps can it be absolutely determined. But on the whole it is more reasonable to think that the same plan of formation is continued from the beginning, than that at any subsequent period there is a change in that plan. It may be observed, that it is exactly the same creative action which produces the natural structure, or any deviation from it; for in cases of deviation the action is either carried too far, ceases too soon, or is diverted into uncommon channels. This will explain the various kinds of monstrosity from redundancy, deficiency, or transposition of parts.

XXII. On the Georgian Planet and its Satellites. By Wm. Herschel, LL. D., F. R. S. p. 364.

In a paper, containing an account of the discovery of 2 satellites revolving round the Georgian planet, Dr. H. gave the periodical times of these satellites in a general way, and added that their orbits made a considerable angle with the ecliptic. The most convenient way of determining the revolution of a satellite round its primary planet, which is that of observing its eclipses, cannot now be used with the Georgian satellites; and as to taking their situations in many successive oppositions of the planet, which is also another very eligible method, that must of course remain to be done at proper opportunities. The only way then left, was to take the situations of these satellites, in any place where they could be ascertained with some degree of precision, and to reduce them afterwards by computation to such other situations as were required for the purpose. In Jan. Feb. and March, 1787, the positions were determined by causing the planet to pass along a wire, and estimating the angle a satellite made with this wire, by a high magnifying power. But then he could only use such of these situations where the satellite happened to be either directly in the parallel of declination, or in the meridian of the planet; or where, at least, it did not deviate above a few degrees from either of them; as it would not have been safe to trust to more distant estimations.

In computing the periods of the satellites Dr. H. contented himself with synodical appearances, as the position of their orbits, at the time when the situations were taken from which these periods are deduced, was not sufficiently known to attempt a very accurate sidereal calculation. By 6 combinations of positions at a distance of 7, 8, and 9 months of time, it appears that the first satellite performs a synodical revolution round its primary planet in $8^d\ 17^h\ 1^m$ and 19.3^s. The period of the 2d satellite deduced likewise from 4 such combinations, at the same distance of time, is $13^d\ 11^h\ 5^m$ and 1.5^s. The combinations of which the above quantities are a mean do not differ much among themselves; it may there-

fore be expected that these periods will come very near the truth; and indeed Dr. H. for many months after used to calculate the places of the satellites by them, and always found them in the situations where these computations gave reason to expect to see them. The epochæ, from which astronomers may calculate the positions of these satellites, are Oct. 19, 1787; for the first $19^h\ 11^m\ 28^s$; and for the 2d $17^h\ 22^m\ 40^s$. They were at those times 76° 43′ north following the planet; which is the place of the greatest elongation of the 2d satellite; where consequently its real angular situation is the same as the apparent one.

The next thing to be determined in the elements of these satellites, is their distance from the planet; and as we know that, when the periodical times are given, it is sufficient to have the distance of one satellite, in order to find that of any other, he confined his attention to the discovery of the distance of the 2d. As soon as he attempted measures, it appeared that the orbit of this satellite was seemingly elliptical; it became therefore necessary, in order to ascertain its greatest elongation, to repeat these measures in all convenient situations; the result of which was, that on the 18th of March, at $8^h\ 2^m\ 50^s$, he found the satellite at the distance of 46″.46; this being the largest of all the measures he had an opportunity of taking. Hence by computation it appears, that the satellite's greatest visible elongation from its planet, at the mean distance of the Georgium Sidus from the earth, will be 44″.23. Admitting therefore at present, that the satellite moves in a circular orbit about its planet, we cannot be much out in taking the calculated quantity of 44″.23 for the true measure of its distance. And, having ascertained this point, we calculate, by the law of Kepler, and the assigned period of the first satellite, that its distance from the planet must be 33″.09.

As we are now on the subject of such parts of the theory of planets as may be determined by calculation, it will not be amiss to see how the quantity of matter and density of our new planet will stand, when compared with the tables that have been given of the same in the other planets; and in order to this, let us admit the following data as a foundation for our computation, viz. The parallax of the sun 8″.63. The parallax of the moon 57′ 11″.

Its sidereal revolution round the earth $27^d\ 7^h\ 43^m\ 11^s.6$.

The mean distance of the Georgian planet from the sun 19.0818.

The mean distance of its 2d satellite from the planet 44″.23.

The periodical time of this satellite $13^d\ 11^h\ 5^m\ 1^s.5$.

Hence we find that a spectator, removed to the mean distance of the Georgian planet from the earth, would see the radius of the moon's orbit under an angle of 27″.1866; and if 1, d, t, represent the quantity of matter in the earth, the distance of the moon, and its periodical time; also M, D, T, be made to stand for the same things in our new planet and its 2d satellite, we obtain, by known

principles, $M = \frac{t^2 D^3}{T^2 d^3}$. And consequently the quantity of matter in the Georgian planet, is to that contained in the earth, as 17.740612 to 1.

In order to calculate the density, Dr. H. compares the mean of the 4 bright measures of the planet's diameter 3″.7975 to the mean of the 2 dark ones 4″.295; as they are given in his paper on the diameter and magnitude of the Georgium Sidus, in vol. 73 of the Philos. Trans. Whence we obtain another mean diameter 4″.04625; which is probably the most accurate of any yet ascertained. Let us now suppose this measure to belong to the situations of the earth and of the new planet as they were at 10 o'clock, Oct. 25, 1782; which is about the middle of the several times when those measures from which this is deduced were taken. Then by the tables we compute the distance of the two planets from the sun and the angle of commutation; whence, by trigonometry, we find the distance of our new planet from the earth for the supposed 25th of October; and thence deduce its mean diameter, which is 3″.90554. This, when brought to what it would appear if it were seen from the sun at the earth's mean distance, gives 1′ 14″.5246; which, compared with 17″.26, the earth's mean diameter, is as 4.31769 to 1. The Georgium Sidus therefore, in bulk, is 80.49256 times as large as the earth; and consequently its density less than that of the latter in the ratio of .220401 to 1. Also the force of gravity, on this planet's surface, is such as will cause a heavy body to fall through 15¼ feet in one second of time.

It remains now only, in order to complete our general idea of the Georgian planet, to investigate the situation of the orbits of its satellites. It has before been remarked, that when Dr. H. came to examine the distance of the 2d, he perceived immediately that its orbit appeared considerably elliptical. This induced him to attempt as many measures as possible, that he might be enabled to come at the proportion of the axes of the apparent ellipsis; and thence argue its situation. But here he met with difficulties that were indeed almost insurmountable. The uncommon faintness of the satellites; the smallness of the angles to be measured with micrometers which required light enough to see the wires; the unwieldy size of the instrument, which, though very manageable, still demanded assistant hands for its movements, and consequently took away a great share of his own directing power, a thing so necessary in delicate observations; the high magnifiers he was obliged to use, by way of rendering the spaces and angles to be measured more conspicuous; in short, every circumstance seemed to conspire to make the case a desperate one. Add to this, that no measure could possibly succeed which had not the most beautiful sky in its favour; and we may easily judge how scarce the opportunities of taking such measures must be in the variable climate of this island. As far then as a small number of select measures will permit, which, out of about 21 that were taken, amounts only to 5, he enters on the subject of the position of the 2d satellite's orbit.

The following table contains in the first column the correct mean time when the measures were taken. The 2d gives the quantity of these measures. In the 3d column are the same measures reduced to the mean distance of the Georgian planet from the earth. The 4th contains the calculated positions of the satellite as it would have appeared to be situated if it had moved in a circular orbit at rect-angles to the visual ray; and the degrees are numbered from the first observation supposed to have been at zero, and are carried round the circle from right to left.

March	18^d	8^h	2^m	50^s	46″.46	44″.23	0°	0′
	19	7	47	59	44 .24	42 .15	26	28
	20	7	44	8	40 .23	38 .37	53	8
April	11	9	18	27	35 .32	34 .35	283	13
Nov.	9	15	56	15	44 .89	42 .88	199	59

In the use of this table Dr. H. partly contents himself with the construction of a figure, and only applies calculation to the most material circumstances. And from the whole calculations is inferred the following snmmary of results.

The first satellite revolves round the Georgian planet in $8^d\ 17^h\ 1^m\ 19^s$.—Its distance is 33″.—And on Oct. 19, 1787, at $19^h\ 11^m\ 28^s$, its position was 76° 43′ north following the planet.

The 2d satellite revolves round its primary planet in $13^d\ 11^h\ 5^m\ 1.5^s$.—Its greatest distance is 44″.23.—And on Oct. 19, 1787, its position at $17^h\ 22^m\ 40^s$, was 76° 43′ north following the planet. Last year its least distance was 34″.35; but the orbit is so inclined, that this measure will change very considerably in a few years, and by that alteration we shall know which of the double quantities set down for the inclination and node of its orbit are to be used.

The orbit of the 2d satellite is inclined to the ecliptic $\left\{\begin{matrix} 91^\circ\ 1'\ 32''.2 \\ 89\ \ 48\ \ 27.5 \end{matrix}\right\}$; its ascending node is in $\left\{\begin{matrix} 18^\circ \text{ of Virgo} \\ 6 \text{ of Sagittarius} \end{matrix}\right\}$. When the planet passes the meridian, being in the node of this satellite, the northern part of its orbit will be turned towards the $\left\{\begin{matrix} \text{east} \\ \text{west} \end{matrix}\right\}$. The situation of the orbit of the first satellite does not seem to differ materially from that of the 2d. We shall have eclipses of these satellites about the year $\left\{\begin{matrix} 1799 \\ 1818 \end{matrix}\right\}$, when they will appear to ascend through the shadow of the planet almost in a perpendicular direction to the ecliptic.

The satellites of the Georgian planet are probably not less than those of Jupiter.

The diameter of the new planet is 34217 miles.

The same diameter seen from the earth, at its mean distance, is 3″.90554.

From the sun, at the mean distance of the earth, 1′ 14″.5246.

Compared to that of the earth as 4.31769 to 1.

This planet in bulk is 80.49256 times as large as the earth.

Its density as .220401 to 1.

Its quantity of matter 17.740612 to 1.

And heavy bodies fall on its surface 15 feet 3¼ inches in 1 second of time.

XXIII. Experiments on the Formation of Volatile Alkali, and on the Affinities of the Phlogisticated and light Inflammable Airs. By William Austin, M. D. p. 379.

In the former part of the year 1787 Dr. A. undertook to examine the elastic fluid produced on decomposing volatile alkali by the electric stroke, as first suggested by Dr. Priestley. Some alkaline air being thus decomposed, and all its inflammable part separated by combustion in glass vessels inverted in quicksilver, he observed a considerable remainder of phlogisticated air; and after many accurate experiments was fully convinced, that this phlogisticated air had made a part in the constitution of the alkali. This discovery induced him to make a variety of synthetical experiments on the phlogisticated and light inflammable airs, with the hopes of forming volatile alkali from its simple elements.

First, he endeavoured to combine the phlogisticated and light inflammable airs, by mixing them together in various proportions in their elastic state, and adding to them such substances as he thought likely to promote their uniting and forming an alkali. With this view, he threw up to the mixture of these airs, marine acid air, the marine and vitriolic acids, to which he also joined alkaline air. He tried the effect of cold on these mixtures, by applying to the tubes containing them clothes moistened with ether. He even passed the electric spark repeatedly through them, though with little probability of success. Lastly, he decomposed alkaline air, and tried to re-unite the identical parts which formed it by similar additions; but he could not perceive, that in any instance, volatile alkali was produced from its 2 constituent parts mixed together in their simple aëriform state.

Yet it is well known, that these 2 bodies unite very readily, when they are not in an elastic state. An unexpected appearance of volatile alkali had been observed by Dr. Priestley and Mr. Kirwan before we were acquainted with its constitution, and by M. Haussman since this discovery of M. Berthollet. An experiment was exhibited before several gentlemen at Sir Joseph Banks's house, some years ago, in which the quantity of volatile alkali produced is very remarkable. In this experiment a few ounces of powdered tin are moistened with some moderately strong nitrous acid, and after they have stood together a minute or two, about ½ an ounce of fixed alkali is mixed with them. A very pungent smell of volatile alkali is immediately perceived. The experiment succeeds equally, if lime be used instead of fixed alkali. Any person, who moistens a drachm or 2 of filings of zinc with a solution of cupreous nitre; and after they

begin to act on each other adds to them a little salt of tartar, will find volatile alkali to be produced. Nitrous acid, or cupreous nitre, mixed with iron filings, sulphur, and a little water, and kept in a close vessel for some hours, yields a smell of volatile alkali; and if a piece of paper, stained with a vegetable blue substance, be thrown into the vessel, it will soon be turned to a green colour. In each of these experiments the nitrous acid and the water are decomposed. Dephlogisticated air from each of them combines with the metal, and their other constituent parts, the phlogisticated air of the acid, and inflammable air of the water, being disengaged at the same instant, unite and form volatile alkali. Many other similar experiments might be mentioned; but these are abundantly sufficient to prove, that if phlogisticated and light inflammable air be presented to each other at the instant of their separation from solid or liquid substances, and before their particles have receded from each other, they readily combine and generate volatile alkali.

That these two substances do not combine in their elastic state, seems to be owing principally to the inflammable air. When these 2 airs combine, it seems necessary that they part with a certain quantity of that fire to which they owe their elasticity; and that, unless their attraction to each other exceed their attraction to fire, they will not unite. Even when they are combined in the form of volatile alkali, if heat be applied, they immediately recede from each other, and the alkali is decomposed. When they are not in an aëriform state their attraction to each other is greater, on account of the proximity of their parts; it is then superior to their attraction to fire, and therefore they combine; but when their particles have receded from each other, as in the aëriform state, their attraction to each other is so diminished by the distance of their parts, that their attraction to fire, which is uniform, prevails, and keeps them in a separate state. The specific gravity of inflammable air being 11 times less than that of phlogisticated air the distance of its particles must be greater than the distance of the particles of phlogisticated air, in the proportion of $\sqrt[3]{11}$ to 1, if the elementary particles of the 2 airs be of equal magnitude; and its effect, on this account, in diminishing attraction, must be greater than that of phlogisticated, in the proportion of those numbers, or more probably as the squares.

Into a cylindrical glass tube, filled with, and inverted in, quicksilver, Dr. A. introduced some phlogisticated air, and afterwards some iron filings moistened with distilled water. By this arrangement light inflammable air, which is given out from water in contact with iron filings, meeting with phlogisticated air at the instant of its extrication, combines with it, and forms volatile alkali. In order to detect the minute quantities of volatile alkali, which were thus generated, he fixed to the inside of the glass tube a small piece of paper, stained with the rind of the blue raddish. The vegetable blue was in 24 hours changed to a

green colour. As an additional proof of the production of volatile alkali, he kept in the same tube some paper, which had been dipped in a solution of cupreous nitre, expecting to see its colour changed from green to blue, by the alkali which was to be produced. The green paper became gradually paler, and in a few days the blue colour appeared. This experiment affords a very satisfactory demonstration of the formation of volatile alkali. Water and iron filings mixed together yield inflammable air; but if this be given out in contact with phlogisticated air, volatile alkali is produced. In these circumstances a double attraction takes place: one part of the water is attracted by the iron; the other is attracted by the phlogisticated air; and the water seems by these compound affinities to be much more rapidly decomposed, than when iron and water are mixed by themselves.

Volatile alkali is formed in a very few hours, if nitrous air be used instead of the phlogisticated, all other circumstances remaining as in the former experiment. When Dr. A. used nitrous acid not well freed from its acid, by which the vegetable blue colour has been turned red, a sufficient quantity of alkali has been generated in 24 hours to change it to a green. If iron filings and water be exposed to nitrous air for a considerable time, the nitrous air is so altered that a candle burns in it with increased brightness, as was observed by Dr. Priestley. This change is accounted for by the formation of the alkali, which depriving the nitrous air of its phlogisticated part, leaves a greater proportion of dephlogisticated air.

This experiment also succeeds in atmospheric air, though a longer time is necessary to produce a sensible alteration in the colours employed as tests of the alkali; but the change is very evident in a day or two. Hence we may conclude, that whenever iron rusts in contact with water in the open air, or in the earth, volatile alkali is formed. Phlogisticated air is present in all parts of the terràqueous globe, and operations are constantly going on, by which inflammable air is separated from water, and perhaps from other bodies. Thus we may account for the frequent appearance of volatile alkali in the earth, particularly where inflammable matters abound, among coals and volcanic productions, as also in animal and vegetable substances.

When iron, water, and sulphur act on each other in atmospheric air, volatile alkali is produced. The eudiometer recommended by Scheele is, for this reason, incorrect. Some phlogisticated air disappears, and volatile alkali is formed. This method therefore seems to have misled that great chemist in his analysis of the atmosphere, and induced him to suppose, that the quantity of phlogisticated air in the atmosphere is only $2\frac{2}{3}$ times that of dephlogisticated air.

There is a combination of light inflammable air with sulphur forming hepatic air. It has been observed by the celebrated Mr. Kirwan, that if nitrous air be

mixed with hepatic air, volatile alkali will be formed. Dr. A. often repeated this experiment, and marked the formation of the volatile alkali by the change of the vegetable blue to a green colour. In hepatic air the parts of inflammable air are brought nearer to each other than they are in their simple aëriform state,* and therefore the phlogisticated air of the nitrous air combines with them, and generates volatile alkali.

From all these experiments it follows, that whether phlogisticated air be in a state of purity, or mixed with dephlogisticated air, as in the atmosphere, or combined with it as in nitrous air, it will in either case unite with the gravitating matter of light inflammable air, provided this substance be presented to it in a state of condensation; but if the circumstances be reversed, the same combination does not take place. No union is formed between inflammable air and the phlogisticated part of nitrous air, even though marine acid be added, which, by its attraction to dephlogisticated air, would contribute to decompose the nitrous air, and by its attraction to volatile alkali would tend to unite its constituent parts: or if to light inflammable air we add nitrous air and iron filings, no combination ensues; though it has been often observed that volatile alkali is readily generated, when nitrous air is presented to the inflammable at the instant of its extrication from water and iron.

The proportions of the phlogisticated and inflammable airs in volatile alkali, as discovered by calculation, approach very near to the result of M. Berthollet's experiments. If we take the specific gravities of these airs, given in Mr. Kirwan's late publication.

100 cubic inches contain 18.16 grains of alkaline air.
.... 30.535 .... of phlogisticated air.
.... 2.613 .... of inflammable air.

According to M. Berthollet alkaline air is expanded on decomposition from 1.7 to 3.3. Its specific gravity after decomposition must therefore be lessened in the same proportion; and 100 cubic inches will be found to contain only 9.355 grains of alkaline air thus expanded. In what proportion must the phlogisticated and inflammable airs be, in order to form a mixture of this specific gravity?

Let x represent the number of grains of phlogisticated air in 100 cubic inches of the mixture: then $9.355 - x$ will express the number of grains of inflammable air. As the weight of 1 cubic inch is to a cubic inch, so will the

* After these experiments were made, Dr. A. found that this is not the case. The electric spark decomposes hepatic air, and leaves a quantity of inflammable air equal in bulk to the hepatic air very nearly. However, as the inflammable air leaves the sulphur on the application of the electrical spark, it should seem that the proper matter of inflammable air is more disposed to combine with fire than with sulphur; which may be the reason why hepatic air is decomposed by nitrous air, while pure inflammable air is not affected by it.—Orig.

weight of either air in the mixture be to the cubic inches of that air in the mixture; and therefore .30535 the weight of a cubic inch of phlogisticated air, shall be to 1, as x is to $\frac{x}{.30535}$ which must be the number of cubic inches of phlogisticated air in 100 cubic inches of the mixture; and the weight of a cubic inch of inflammable air, that is, $.02613 : 1 :: 9.355 - x : \frac{9.355 - x}{.02613}$ the cubic inches of inflammable air in 100 cubic inches of the mixture. Thus we have an expression for the cubic inches of each air; these two quantities taken together are equal to 100 cubic inches by supposition; from which equation is found $x = 7.373$, the number of grains of phlogisticated air in 100 cubic inches, or in 9.355 grains of the mixture; and $9.355 - 7.373 = 1.982$, the grains of inflammable air. Now $7.373 : 1.982 :: 121 : 32$; and the quantity of phlogisticated air is to that of inflammable air, as 121 to 32.

According to M. Berthollet's experiments, the quantity of phlogisticated is to that of inflammable air, as 121 : 29. This is not very wide of calculation. If we consider the great difficulty of obtaining these specific gravities with exactness, we must be pleased to find so near a concurrence, and place more confidence in experiments on the specific gravities and combinations of aëriform bodies, than has generally been given them. M. Berthollet's experiments come within $\frac{1}{10}$ of calculation; and this difference will be diminished by $\frac{1}{30}$ if we take the specific gravities of the phlogisticated and inflammable airs in the proportion of 11 to 1, as he has done, instead of Mr. Kirwan's proportion, which Dr. A. followed in this calculation.

XXIV. Some Properties of the Sum of the Divisors of Numbers. By Edward Waring, M. D., F. R. S. p. 388.

1. Let the equation $x - 1 \,.\, x^2 - 1 \,.\, x^3 - 1 \,.\, x^4 - 1 \,.\, x^5 - 1 \ldots x^n - 1 = x^h - px^{h-1} + qx^{h-2} - rx^{h-3} + sx^{h-4} - \&c. = x^h - x^{h-1} - x^{h-2} + x^{h-5} + x^{h-7} - x^{h-12} - x^{h-15} + x^{h-22} + x^{h-26} - x^{h-35} - x^{h-40} + x^{h-51} + x^{h-57} - \&c. \ldots x^{h-n} \pm \&c. = A = 0$. The signs + and − proceed alternately by pairs unto the term x^{h-n}. The co-efficients of all the terms to the above-mentioned (x^{h-n}) will be $+1$, -1 or 0; they will be $+1$ when multiplied into x^{h-v}, where $v = \frac{3z^2 + z}{2}$ or $= \frac{3z^2 - z}{2}$, and z an even number; but -1, if z be an uneven number; in all other cases they will be $= 0$. The numbers 1, 2, 5, 7, 12, 15, 22, 26, 35, 40, &c. subtracted from h, may be collected from the addition of the numbers 1, 1, 3, 2, 5, 3, 7, 4, 9, 5, 11, 6, &c. which consist of two arithmetical serieses 1, 3, 5, 7, 9, 11, &c. 1, 2, 3, 4, 5, 6, 7, &c. intermixed.

2. The sum of any power (m) of each of the roots in the equation $A = 0$ will

be s (m), where s (m) denotes the sum of all the divisors of the number m, if m be not greater than n.

Cor. Hence (by the rule for finding the sum of (m) powers of each of the roots from the sum of the inferior powers and co-efficients of the given equation) may be deduced s $(m) = p$s $(m-1) - q$s $(m-2) + r$s $(m-3) - s$s $(m-4) + t$s $(m-5)$ — &c. $=$ s $(m-1) +$ s $(m-2) -$ s $(m-5) -$ s $(m-7) +$ s $(m-12) +$ s $(m-15) -$ s $(m-22) - (m-26)$ + &c. which is the property of the sum of divisors invented by the late M. Euler.

Cor. By substituting for s $(m-1)$, s $(m-2)$, &c. their values s $(m-2) +$ s $(m-3) -$ s $(m-6) -$ s $(m-8)$ + &c., s $(m-3) +$ s $(m-4) -$ s $(m-7) -$ s $(m-9)$ + &c. &c. in the given equation s $(m) =$ s $(m-1) +$ s $(m-2) -$ s $(m-5) -$ s $(m-7)$ + &c. may be acquired an expression for the sum s (m) in terms of the sums of the divisors of numbers less than $m-1$, $m-2$, &c.: the same method may be used for a similar purpose in some of the following propositions.

Cor. By the rule for finding the sum of the contents of every (m) roots from the sums of the powers of each of the roots, may be deduced the equation $\pm$ $1 \cdot 2 \cdot 3 \cdot 4 \ldots m$, or

$$0 = 1 - m \cdot \frac{m-1}{2} \text{s}(2) + m \cdot m-1 \cdot \frac{m-2}{3} \text{s}(3)$$
$$- m \cdot m-1 \cdot m-2 \cdot \frac{m-3}{4} \text{s}(4) + \text{\&c.}$$
$$+ m \cdot m-1 \cdot \frac{m-2}{2} \cdot \frac{m-3}{2^2} \text{s}((2))^2 - \text{\&c.}$$

in which the sum of the divisors of any number m is expressed by the sums of the divisors of the inferior numbers $m-1$, $m-2$, &c. and their powers. If v be an even number, then $\pm 1 \cdot 2 \cdot 3 \ldots m$ will have the same sign as the co-efficient; if uneven, the contrary; but if the co-efficient $= 0$, then will the content $1 \cdot 2 \cdot 3 \ldots m$ vanish. The law of this series is given in the Meditationes Algebraicæ.

3. Let H be the number of different ways by which the sum of any two numbers 1, 2, 3, 4, … $m-2$, $m-1$, can become $= m$; H′ the number of ways by which the sum of any 3 of the above-mentioned numbers can make m; H″, H‴, H⁗, &c. the number of ways by which the sum of any 4, 5, 6, &c. of the above-mentioned numbers is $= m$ respectively; then will $1 - \text{H} + \text{H}' - \text{H}'' + \text{H}'''$ — &c. $= \pm 1$ or 0. Let $m = \frac{3z^2 \pm z}{2}$, and it will be $+1$ or -1, according as z is an odd or even number; in all other cases it will be $= 0$.

PART 2.—1. Let the equation be $x-1 \cdot x^2-1 \cdot x^3-1 \cdot x^5-1 \cdot x^7-1 \cdot x^{11}-1 \cdot x^{13}-1 \cdot x^{17}-1 \ldots x^n-1$. &c. $= x^{b'} - px^{b'-1} + qx^{b'-2} - rx^{b'-3} + sx^{b'-4}$ — &c. $= x^{b'} - x^{b'-1} - x^{b'-2} + x^{b'-4} + x^{b'-8} - x^{b-10} - x^{b'-11} + x^{b'-12} + x^{b'-16} - x^{b'-17} - x^{b'-19} + x^{b'-20} - x^{b'-23} + 2x^{b'-24} - x^{b'-26} - x^{b'-27} +$

$x^{b'-28}$, &c. $= A' = 0$; the sum of any power (m) of each of the roots in the equation $A' = 0$ will be $s'(m)$, where $s'(m)$ denotes the sum of all the prime divisors of the number m, and m is not greater than n.

Cor. Hence, by the rule before-mentioned $s'(m) = s'(m-1) + s'(m-2) - s'(m-4) - s'(m-8) + s'(m-10) + s'(m-11) - s'(m-12) - s'(m-16) + s'(m-17) + s'(m-19) - s'(m-20) + s'(m-23) - 2s'(m-24) + s'(m-26) + s'(m-27) - s'(m-28) + s'(m-29)$, &c.

If in this, or the preceding, or subsequent analogous cases $s(m-r)$, or $s'(m-r)$, or $s^l(m-r)$, becomes $s(0)$, or $s'(0)$, or $s^l(0)$; for $s(0)$, or $s'(0)$, or $s^l(0)$, always substitute r.

Cor. Let L be the co-efficient of the term $x^{b'-m}$; then, by the above-mentioned series contained in the Meditationes Algebraicæ, will $1 . 2 . 3 . 4 \ldots\ldots m \times L = 1 - m . \frac{m-1}{2} s'(2)$

$+ m . m-1 . \frac{m-2}{3} \times s'(3) - m . m-1 . m-2 . \frac{m-3}{4} \times s'(4)$

$+$ &c. $\qquad + m . m-1 . m-2 . \frac{m-3}{8} \times s'((2))^2$

$-$ &c. be an equation, which expresses a relation between the prime divisors of the numbers $1, 2, 3, 4 \ldots m-1, m$, and their powers.

Cor. The co-efficient $L =$ the difference between the two respective numbers of different ways that m can be formed by adding the prime numbers 1, 2, 3, 5, 7, 11, 13, 19, &c. the one with, and the other without, 2.

PART 3.—1. Let an equation $x^\alpha - 1 . x^\beta - 1 . x^\gamma - 1 . x^\delta - 1 \times$ &c. $= x^b - px^{b-1} + qx^{b-2} - rx^{b-3} +$ &c. $= 0$; then will the sum of the (m) powers of each of its roots be the sum of all the divisors of m, that can be found among the numbers $\alpha, \beta, \gamma, \delta$, &c.

2. The co-efficient of the term x^{b-m} will be the difference between the two respective numbers of different ways, that the number (m) can be formed from the addition of the numbers $\alpha, \beta, \gamma, \delta$, &c.; the one containing in it an odd number of the even numbers contained in $\alpha, \beta, \gamma, \delta$, &c.; the other not.

PART 4.—1. Let $x^l - 1 . x^{2l} - 1 . x^{3l} - 1 . x^{4l} - 1 \ldots\ldots x^{nl} - 1$. &c. $= x^b - px^{b-l} + qx^{b-2l} - rx^{b-3l} +$ &c. $= x^b - x^{b-l} - x^{b-2l} + x^{b-5l} + x^{b-7l} - x^{b-12l} - x^{b-15l} +$ &c. $= B = 0$, of which equation all the co-efficients are the same as in case the first, and consequently ± 1 or 0 to the term (x^{b-nl}).

2. The sum of any power $l \times m$ of each of the roots of the equation $B = 0$ will be $s^l(m)$; where $s^l(m)$ denotes the sum of the divisors of m, which are divisible by l.

Cor. Hence $s^l(m) = s^l(m-l) + s^l(m-2l) - s^l(m-5l) - s^l(m-7l)$

$+ s^{l}(m - 12l) + s^{l}(m - 15l) - s^{l}(m - 22l) - s^{l}(m - 26l) +$ &c.; the law of the series has been given in case 1.

Cor. The sum of all the divisors of m not divisible by $l = s(m) - s^{l}(m) = s(m - 1) - s^{l}(m - l) + (s(m - 2) - s^{l}(m - 2l)) - s(m - 5) - s^{l}(m - 5l)) - (s(m - 7) - s^{l}(m - 7l)) +$ &c.

A similar rule may be predicated of the sum of the divisors not divisible by the numbers a, b, c, d, &c.: for the sum of the divisors of the number (m) divisible by a, b, c, d, e, &c., where a, b, c, d, e, &c are prime to each other $= (s^{a}(m) + s^{b}(m) + s^{c}(m) + s^{d}(m) + s^{e}(m) +$ &c.)) $- ((s^{a\times b}(m) + s^{a\times c}(m) + s^{b\times c}(m) + s^{a\times d}(m) + s^{b\times d}(m) + s^{\times d}(m) + s^{a\times e}(m) +$ &c.)) $+ (s^{a\times b\times c}(m) - s^{a\times b\times d}(m) + s^{a\times b\times d}(m) + s^{b\times c\times d}(m) + s^{a+b+c}(m) +$ &c.)) $- ((s^{a+b+c+d}(m) + s^{a+b+c+e}(m) +$ &c.)) $+ (s^{a+b+c+d+e}(m) +$ &c.)) $-$ &c. $= l =$ the sum of all the divisors of m .... divisible by a, b, c, d, e, &c. respectively added together, $-$ the sum of all the divisors of m divisible by the products (ab, ac, bc, &c.) of any 2 of the quantities a, b, c, d, &c. $+$ the sum of all the divisors of m divisible by the contents (abc, abd, acd, bcd, &c.) of every 3 of the quantities a, b, c, d, &c. $-$ the sum of all the divisors of m divisible by the contents of every 4 of the above-mentioned quantities a, b, c, d, &c. $+$ and so on, and consequently $s(m) - c$ is the sum required.

The principles given in the former parts may be applied to this, and extended to equations of which the factors have the formula $x^{a} \pm k$; and from the sum of the inferior powers of each of the roots, and the co-efficients, may be collected the sum of the superior; the same may be performed by the co-efficients only, &c.

PART 5.—1. $S(\alpha \times \beta) = \alpha \times s^{l}(\beta) +$ sum of all the divisors of β not divisible by $\alpha = \beta \times s(\alpha) +$ sum of all the divisors of α not divisible by β.

2. $S^{l}(\alpha \times \beta) = \alpha \times s^{l}(\beta) +$ sum of all the divisors of β divisible by l but not by $\alpha =$ &c.

3. $S(\alpha \times \beta \times \gamma \times \delta \times$ &c.) $= \alpha \times s(\beta \times \gamma \times \delta \times \epsilon$, &c.) $+$ sum of all the divisors of $\beta \times \gamma \times \delta \times \epsilon$, &c. not divisible by $\alpha = \alpha \times \beta \times s(\gamma \times \delta \times \epsilon$, &c.) $+$ sum of all the divisors of $\beta \times \gamma \times \delta \times \epsilon$, &c. not divisible by $\alpha + \alpha \times$ sum of all the divisors of $\gamma \times \delta \times \epsilon$, &c. not divisible by $\beta = \alpha \times \beta \times \gamma \times s(\delta \times \epsilon$, &c.) $+$ sum of all the divisors of $\beta \times \gamma \times \delta \times \epsilon$, &c. not divisible by $\alpha + \alpha \times$ sum of all the divisors of $\gamma \times \delta \times \epsilon$, &c. not divisible by $\beta + \alpha \times \beta \times$ sum of all the divisors of $\delta \times \epsilon$, &c. not divisible by $\gamma = \alpha \times \beta \times \gamma \times \delta \times s(\epsilon$, &c.) $+$ sum of all the divisors of $\beta \times \gamma \times \delta \times \epsilon$, &c. not divisible by $\alpha + \alpha \times$ sum of all the divisors of $\gamma\delta\epsilon$, &c. not divisible by $\beta + \alpha \times \beta \times$ sum of all the divisors of $\delta\epsilon$, &c. not divisible by $\gamma + \alpha \times \beta \times \gamma \times$ sum of all the divisors of ϵ, &c. not divisible by $\delta =$ &c. The law of the series is manifest. The letters $\alpha, \beta, \gamma, \delta$, &c. which are not contained between the parentheses, denote prime numbers.

Cor. If some of the letters α, β, γ, δ, &c. be substituted for others, and others for them, the equations resulting will be just, and consequently many new equations may be deduced. If in the preceding equations for s be written s′, and for the sum of all the divisors of a certain quantity not divisible by a prime number (α, or β, or γ, &c.) be written the sum of all the divisors of that quantity not divisible by the same prime number, but divisible by l; the propositions resulting will be true. These equations may be applied to the equations given in the preceding parts, and from thence many others be deduced.

XXV. Experiments on the Production of Artificial Cold. By Mr. Richard Walker, Apothecary to the Radcliffe Infirmary at Oxford. p. 395.

Mr. W's most powerful frigorific mixture is the following: Of strong fuming nitrous acid, diluted with water (rain or distilled water is best) in the proportion of 2 parts of the former to 1 of the latter, each by weight, well mixed, and cooled to the temperature of the air, 3 parts; of vitriolated natron (Glauber's salt) 4 parts; of nitrated ammonia (nitrous ammoniac) 3¼ parts; each by weight, reduced separately to fine powder: the powdered vitriolated natron is to be added to the diluted acid, the mixture well stirred, and immediately afterward the powdered nitrated ammonia, again stirring the mixture: to produce the greatest effect, the salts should be procured as dry and transparent as possible, and used freshly powdered. These seem to be the best proportions when the temperature of the air and ingredients is + 50°; as the temperature at setting out is higher or lower than this, the quantity of the diluted acid will evidently require to be proportionably diminished or increased. This mixture is but little inferior to one made by dissolving snow in nitrous acid, for it sunk the thermometer from + 32° to — 20°; perhaps it may be possible to reduce the salts to so fine a powder as to make it equal. In this last experiment the diluted acid was equal in quantity to the vitriolated natron, being 4 parts each, the nitrated ammonia 3¼ as before. A powder composed of muriated ammonia (crude sal ammoniac) 5 parts, nitrated kali (nitre) 4 parts, mixed, may be substituted in the stead of nitrated ammonia, with nearly equal effect, and in the same proportion.

Crystallized nitrated ammonia, reduced to very fine powder, sunk the thermometer, during its solution in rain water, 48°, from + 56°, the temperature of the air and materials, to + 8°; and when evaporated gently to dryness, and finely powdered, it sunk the thermometer 49°, to + 7°, the temperature of the air and materials being as before at + 56°: therefore, in this salt (which produces, as appears above, much greater cold during solution in water, than any other hitherto known) the water of crystallization is not in the least conducive to that effect. Mr. W. expected, that by diluting the strong nitrous acid to the proper strength with snow, instead of water, by which its temperature

would be much reduced, and then adding the salts, a much greater degree of cold might be produced; but, by various diversified trials, but little advantage was gained. In the course of this winter, some diluted nitrous acid, in a wide mouthed phial, was immersed in a freezing mixture; when cooled to about — 32°, it froze intirely to the consistence of an ointment, when the thermometer suddenly rose to — 2°; on adding some snow that lay by, it became again liquid, and the mercury sunk into the bulb of a thermometer graduated to — 76°; he knew not its exact strength; but by the effect imagined it might correspond nearly with that which is capable of the easiest point of spirituous congelation. Cold, he found, may be produced by the union of such salts as on mixing are decomposed, and become liquid or partially so. The mineral alkali produces this effect with all the ammoniacal salts; but with nitrated ammonia to a considerable degree. The mineral alkali added in powder to nitrous acid, diluted as above, sunk the thermometer 22° only, from 53°, temperature of air and materials, to 31°. This salt contains nearly as much water of crystallization as vitriolated natron, and produces more cold during solution in water than that salt. The reason why it produces less when added to acid than the neutral salt does, is perhaps sufficiently evident. He has observed the thermometer to be stationary, or even to rise, during the violent effervesence produced on mixing those materials, and to sink as soon as that ceased.

Vitriolated natron dissolved indifferently in rectified spirit of wine, and produced neither heat nor cold; the disposition to produce cold, during its solution, being perhaps exactly counteracted by the tendency which the dissolved salt hath in uniting with the spirit to produce heat. Vitriolated magnesia, a salt very similar to vitriolated natron, during solution in the diluted nitrous acid, produced nearly as much cold as that salt: the small difference there is between them, as to this effect, may be owing to the former containing rather less water in its crystals.

Vitriolated natron, liquified by heat, was set to cool: when its temperature was reduced to 70°, it became solid, and the thermometer immediately rose to 88°, its freezing point. Does not the quantity of sensible heat evolved by this salt, in becoming solid, indicate its great capacity for heat, in returning to a liquid state, and consequently account in a great measure for its producing such intense cold during solution in the diluted mineral acids? Two salts, vitriolated argillaceous earth (alum) and tartarized natron (Rochelle salt,) each contain nearly as much water of crystallization as vitriolated natron; but produced neither of them any considerable effect during solution in the diluted nitrous acid; the latter made the thermometer rise: neither did their temperatures increase, like that salt, in changing from a liquid to a solid state.

From the obvious application of artificial frigorific mixtures to useful purposes,

in hot climates especially, where the inhabitants scarcely know by the sense of feeling winter from summer, it may not be amiss to hint at the easiest and most economical method of using them. For most intentions perhaps, the following cheap one may be sufficient: of strong vitriolic acid, diluted with an equal weight of water, and cooled to the temperature of the air, any quantity; add to this an equal weight of vitriolated natron in powder: this is the proportion when the temperature set out with is + 50°, and will sink the thermometer to 5°; if higher, the quantity of salt must be proportionably increased. The obvious and best method of finding the necessary quantity of any salt to produce the greatest effect, by solution in any liquid, at any given temperature, is by adding it gradually until the thermometer ceases to sink, stirring the mixture all the while.

If a more intense cold be required, double aqua fortis, as it is called, may be used; vitriolated natron, in powder, added to this, produces very nearly as much cold as when added to the diluted nitrous acid: it requires a rather larger quantity of the salt, at the temperature of + 50°, about 3 parts of the salt to 2 parts of the acid: it will sink the thermometer from that temperature nearly to 0, and the consequence of more salt being required is, its retaining the cold rather longer. This mixture has one great recommendation, a saving of time and trouble. A little water in a phial, immersed in a small tea cup of this mixture, will be soon frozen in summer; and if the salt be added in crystals unpounded to double aqua fortis, even at a warm temperature, the cold produced will be sufficient to freeze water or creams; but if diluted with $\frac{1}{4}$ its weight of water, and cooled, it is about equal to the diluted nitrous acid above-mentioned, and requires the same proportion of the salt. A mixture of vitriolated natron and diluted nitrous acid sunk the thermometer from + 70°, temperature of air and ingredients, to + 10°. The cold in any of these mixtures may be kept up a long time by occasional additions of the ingredients in the proportions mentioned. A chemist would make the same materials serve his purpose repeatedly.

Equal parts of muriated ammonia and nitrated kali in powder make a cheap and convenient composition for producing cold by solution in water; it will, by the following management, freeze water or creams at Midsummer. June 12th, 1787, a very hot day, Mr. W. poured 4 oz. wine measure, of pump water, at the temperature of 50° (it is well known that water at springs retains nearly the same temperature winter and summer, viz. about 50°, to which temperature the water may be reduced during the warmest weather, by pumping off some first) on 3 oz. Avoirdupois weight, of the above powder (previously cooled by immersing the vessel containing it in other water at 50°,) and after stirring the mixture its temperature was 14°; some water contained in a small phial, immersed in this mixture, was consequently soon frozen. This solution was afterwards evaporated to dryness, in an earthen vessel, reduced to powder, and

added to the same quantity of water, under the same circumstances as before, when it again sunk the thermometer to 14°. Since that time he has repeatedly used a composition of this kind for the purpose of producing cold, without observing any diminution in its effect after many evaporations. The cold may be economically kept up and regulated any length of time, by occasionally pouring off the clear saturated liquor, and adding fresh water, observing to supply it constantly with as much of the powder as it will dissolve.

The degree of cold at which water begins to freeze has been observed to vary much; but that it might be cooled 22° below its freezing point was perfectly unknown to him till lately. He filled the bulb of 2 thermometers, one with the purest rain water he could procure, the other with pump water; the water was then made to boil in each, till $\frac{1}{3}$ only remained: these were kept in a frigorific mixture, at the temperature of + 10°, for a much longer time than he thought necessary to cool the water to the same temperature; and by repeated trials he found it was necessary to lower the temperature of the mixture to near + 5°, to make the water in either of them freeze. These were likewise suspended out of doors, close to a thermometer, during the late frost, and the water never observed frozen. On March the 22d, at 6 in the morning, the water in each remained unfrozen, though the tubes were gently shaken, the thermometer standing at that time at 23°. There appeared to be little difference with respect to the degree of cold necessary to freeze the water, whether the tube of the thermometers were open or closed in vacuo (which was very nearly effected by suffering the water to boil up to the orifice of the tube, and then suddenly sealing it) or not, but unboiled water in the same situation froze in a higher temperature.

It is commonly supposed that gentle agitation of any kind will dispose water, cooled below its freezing point, to become ice; but Mr. W. repeatedly cooled rain water and pump water, boiled a long time, and unboiled, in open vessels to 30° or lower, and constantly succeeded, after trying other kinds of agitation in vain, by stirring, or rather scraping gently, the bottom and sides of the vessel containing the water to be frozen, when after some short time small filaments of ice appeared, and by continuing this motion about every part of the vessel beneath the surface of the water, about $\frac{1}{4}$ of the water commonly froze. A slender, pointed glass rod he used for this purpose.

Extract of a second Letter from Mr. Walker.—A more intense cold may be produced by a solution of salts in water in summer, than can be produced by a mixture of snow and salt in winter. To rain water 6 drs. by weight, I added 6 drs. of nitrated ammonia reduced to a very fine powder, which made the thermometer sink from + 50°, temperature of the materials, to 4°; then adding 6 drs. of mineral alkali very finely powdered, the thermometer sunk to − 7°,

that is 57°. It is observable, that in the latter there are 2 causes concur in producing the effect, the liquefaction both of the snow and salt; but in the experiment just mentioned the liquefaction of the salts only. Vitriolated natron, after it had given out its water of crystallization by exposure to the atmosphere, produced no change of temperature by solution in the diluted nitrous acid, but during solution in water produced heat, as did also the mineral alkali.

XXVI. A Description of an Instrument which, by the turning of a Winch, produces the two States of Electricity without Friction or Communication with the Earth. By Mr. William Nicholson. p. 403.

Plate 6, fig. 3, represents the apparatus supported on a glass pillar 6¼ inches long. It consists of the following parts. Two fixed plates of brass, A and C, are separately insulated and disposed in the same plane, so that a revolving plate B may pass very near them, without touching. Each of these plates is 2 inches in diameter; and they have adjusting pieces behind, which serve to place them accurately in the required position. D is a brass ball, also of 2 inches diameter, fixed on the extremity of an axis that carries the plate B. Besides the more essential purpose this ball is intended to answer, it is so loaded within on one side, that it serves as a counterpoise to the revolving plate, and enables the axis to remain at rest in any position. The other parts may be distinctly seen in fig. 4. The shaded parts represent metal and the white represent varnished glass. ON is a brass axis, passing through the piece M, which last sustains the plates A and C. At one extremity is the ball D before-mentioned; and the other is prolonged by the addition of a glass stick, which sustains the handle L and the piece GH separately insulated. B, F, are pins rising out of the fixed plates A and C, at unequal distances from the axis. The cross piece GH, and the piece K, lie in one plane, and have their ends armed with small pieces of harpsichord-wire, that they may perfectly touch the pins BF in certain points of the revolution. There is also a pin I, in the piece M, which intercepts a small wire proceeding from the revolving plate B.

The touching wires are so adjusted, by bending, that when the revolving plate B is immediately opposite the fixed plate A, the cross-piece GH connects the 2 fixed plates, at the same time that the wire and pin at I form a communication between the revolving plate and the ball. On the other hand, when the revolving plate is immediately opposite the fixed plate C, the ball becomes connected with this last plate, by the touching of the piece K against F; the 2 plates, A and B, having then no connection with any part of the apparatus. In every other position the 3 plates and the ball will be perfectly unconnected with each other.

Mr. Cavallo's discovery, so well explained in the last Bakerian lecture, that the minute differences of electrization in bodies, whether occasioned by art or

nature, cannot be completely destroyed in any definite time, may be applied to explain the action of the present instrument. When the plates A and B are opposite each other, the 2 fixed plates A and C may be considered as one mass; and the revolving plate B, together with the ball D, will constitute another mass. All the experiments yet made concur to prove, that these 2 masses will not possess the same electric state; but that, with respect to each other, their electricities will be plus and minus. These states would be simple and without any compensation, if the masses were remote from each other; but as that is not the case, a part of the redundant electricity will take the form of a charge in the opposed plates A and B. From other experiments it appears that the effect of the compensation on plates opposed to each other, at the distance of $\frac{1}{10}$ part of an inch, is such that they require, to produce a given intensity, at least 100 times the quantity of electricity that would have produced it in either, singly and apart. The redundant electricities in the masses under consideration will therefore be unequally distributed: the plate A will have about 99 parts, and the plate C 1; and, for the same reason, the revolving plate B will have 99 parts of the opposite electricity, and the ball D 1. The rotation, by destroying the contacts, preserves this unequal distribution, and carries B from A to C, at the same time that the tail K connects the ball with the plate C. In this situation, the electricity in B acts on that in C, and produces the contrary state, by virtue of the communication between C and the ball; which last must therefore acquire an electricity of the same kind with that of the revolving plate. But the rotation again destroys the contact, and restores B to its first situation opposite A. Here, if we attend to the effect of the whole revolution, we shall find that the electric states of the respective masses have been greatly increased: for the 99 parts in A and in B remain, and the 1 part of electricity in C has been increased so as nearly to compensate 99 parts of the opposite electricity in the revolving plate B, while the communication produced an equal mutation in the electricity of the ball. A 2d rotation will of course produce a proportional augmentation of these increased quantities: and a continuance of turning will soon bring the intensities to their maximum, which is limited by an explosion between the plates.

If one of the parts be connected with an electrometer, more especially that of Bennet, these effects will be very clearly seen. The spark is usually produced by a number of turns between 11 and 20; and the electrometer is sensibly acted on by still fewer. When one of the parts is occasionally connected with the earth, or when the adjustment of the plates is altered, there are some variations in the effects, not difficult to be reduced to the general principles, but sufficiently curious to excite the meditations of persons the most experienced in this branch of natural philosophy. If the ball be connected with the lower part of Bennet's electrometer, and the plate A with the upper part, and any weak electricity be com-

municated to the electrometer, while the position of the apparatus is such that the cross-piece GH touches the 2 pins; a very few turns will render it perceptible. But here, as well as in the common doubler, the effect is rendered uncertain by the condition, that the communicated electricity must be strong enough to destroy and predominate over any other electricity the plates may possess. It scarcely need be observed, that if this difficulty should hereafter be removed, the instrument will have great advantages as a multiplier of electricity in the facility of its use, the very speedy manner of its operation, and the unequivocal nature of its results.

XXVII. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon in Rutland; with the Rain in Hampshire and Surrey, in 1787. Also some Account of the Annual Growth of Trees. By T. Barker, Esq. p. 408.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.				Hampshire.		Surry.
		Highest.	Lowest.	Mean.	Hig.	Low	Mean	Hig.	Low	Mean	Lyndon.	Selbourn	Fyfield.	S. Lamb.
		Inches.	Inches.	Inches	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	30.13	29.11	29.70	45	34½	39	47	25	35	0.415	0.88	0.43	0.60
	Aftern.				46	34½	40	51	31	39½				
Feb.	Morn.	29.85	28.15	29.40	47	41	44	47½	27	39	0.860	3.67	3.40	1.68
	Aftern.				48½	41½	45	53½	37	46				
Mar.	Morn.	30.02	28.47	29.30	49	41	45½	49	31	40	1.782	4.28	3.80	1.62
	Aftern.				50½	42	46½	54	35½	48½				
Apr.	Morn.	30.00	28.54	29.47	50	44	46½	50½	35	42	1.721	0.74	0.69	0.93
	Aftern.				50½	45	47½	56½	42½	50				
May	Morn.	29.86	28.80	29.47	60½	42½	52½	55½	36	48½	1.573	2.06	1.27	1.60
	Aftern.				62	44	54	72½	46½	59				
June	Morn.	29.76	29.05	29.44	63	51½	58	60½	46	54½	1.800	1.50	1.43	0.68
	Aftern.				64½	53	59½	77	55	65½				
July	Morn.	29.93	28.92	29.36	69	56½	60½	66	50½	57	3.169	6.53	3.50	4.12
	Aftern.				70	57	62	79	63	68				
Aug.	Morn.	29.94	28.74	29.53	70	55½	61	63½	49½	56	1.969	0.83	0.74	0.60
	Aftern.				73	56½	63	80	51½	67½				
Sept.	Morn.	30.01	28.59	29.50	60	53½	57	57½	42	51	1.225	1.56	1.47	0.78
	Aftern.				60½	55	57½	65	55	61				
Oct.	Morn.	29.70	28.48	29.26	56	46½	51½	55	36	46	3.726	5.04	3.44	2.41
	Aftern.				57	47	53	61	44½	54½				
Nov.	Morn.	29.95	28.50	29.33	52	34	43	49	20	35½	1.462	4.09	2.57	1.51
	Aftern.				52½	35½	43	54½	30½	42				
Dec.	Morn.	29.93	28.75	29.22	48	34	41	48½	26	37	3.085	5.06	3.48	3.87
	Aftern.				49	35	41½	55½	30½	41				
Means and sums				29.42			50½			49⅓	22.787	36.24	25.82	20.40

On the annual growth of trees.

Oaks.

		Girth. In.		Girth. In.	Rate. In.		Girth. In.	Rate. In.
1	——	——	1772	19	——	1787	41	1.5
2	1758	13	1772	33	1.4	1787	55½	1.5
3	1758	18½	1772	40½	1.6	1787	58	1.2
4	——	——	——	——	——	1787	156½	1.3
5	1758	41	1772	56	1.1	1787	77½	1.4
6	1744	20	1765	45	1.2	1787	74½	1.3

Oaks.

		Girth. In.		Girth. In.	Rate. In.		Girth. In.	Rate. In.			Girth. In.		Girth. In.	Rate. In.		Girth. In.	Rate. In.
7	1758	18	1772	36	1.3	1787	54	1.2	14	1744	21	1765	45	1.1	1787	64	0.9
8	1758	76	1772	93½	1.25	1787	109½	1.1	15	1762	106½	1772	117	1.0	1787	130	0.9
9	1751	124	1772	147	1.1	1787	164½	1.2	16	1751	117	1770	132	0.8	1787	149½	1.0
10	1744	23½	1765	49	1.2	1787	74	1.1	17	1751	114	1770	131½	0.9	1787	145	0.8
11	1744	69½	1772	99	1.1	1787	115	1.1	18	1751	84½	1772	101	0.8	1787	109	0.5
12	1744	14	1765	43	1.4	1787	60	0.8	19	1744	41	1765	58½	0.8	1787	69	0.5
13	1747	82	1765	99½	1.0	1787	120½	1.0									

Ash.

20	—	—	1772	71	—	1787	106	2.3	29	1744	56	1765	77½	1.0	—	—	—
21	1745	23½	1765	67	2.2	1787	111	2.0	30	1755	51½	1772	67	0.9	1787	80	0.9
22	1744	22	1765	55½	1.6	1787	92	1.7	31	—	—	1765	74	—	1781	89	0.9
23	1744	32	1765	61	1.4	1787	94½	1.5	32	1751	45½	1772	67	1.0	1787	77	0.7
24	1744	66	1765	91½	1.4	1787	114	1.0	33	1744	17½	1765	34	0.8	1787	52	0.8
25	1751	20	1772	45	1.25	1782	58½	0.9	34	1744	17	1765	36½	0.9	1787	52½	0.7
26	1765	55	1772	64	1.2	1787	75½	0.9	35	1744	20	1772	40	0.7	—	—	—
27	1747	77	1765	97	1.0	1787	116½	1.0	36	1745	13½	1772	31½	0.7	1787	41	0.6
28	—	—	1772	67½	—	1787	82	1.0									

Elms.

37	1755	0	1772	42	2.5	1787	77	2.3	40	1744	45	1758	58	0.9
38	1744	28	1765	60	1.5	1787	96	1.6	41	1744	48	1758	59	0.8
39	1744	37	1758	50	0.9	1781	72	1.0						

Except the first 2 ash trees, the growth of oak and ash are nearly the same. Mr. B. had some of both sorts planted at the same time, and in the same hedges, of which the oaks are the largest, but there is no certain rule as to that. The common growth of an oak or an ash is about an inch in girth in a year; some thriving ones will grow an inch and a half; the unthriving ones not so much, some probably less than any here, for he chose in general to measure those that seemed thriving.

Large trees grow more timber in a year than small ones; for if the annual growth be an inch, a coat of $\frac{1}{6}$ of an inch thick is laid on all round, and the timber added to the body every year is its length multiplied into the thickness of the coat, and into the girth; and therefore the thicker the tree is, the more timber is added. The body of N° 9 is 9 feet long, the girth under the bark above 13 feet, the thickness of the coat $\frac{1}{6}$ of an inch or $\frac{1}{72}$ of a foot: then $9 \times 13 \times \frac{1}{72}$ is $1\frac{5}{8}$ feet of timber added in a year to the body, besides the increase on all the branches, and it has a very great head; one limb squares 20 inches, and is itself equal to a moderate tree.

The hedge in which N° 4 grows was planted in 1665, probably the tree is not older than that year; it has therefore increased in girth about 1.3 inch every year since it was set. The oak, N° 5, he believed sowed itself; and he did not know there was such a one till about the year 1740, when the hedge being cut, the tree was found, and might be then 20 years old or more. The 2 ash trees N°

20 and 21, grow much faster than any of the rest, but are neither of them handsome growing trees. N° 20 has several seams where the bark is parting from the wood, and are likely to be dead sides. N° 21 was about as thick as a walking-stick in 1730. It does not grow round and smooth, has no dead side, but several deep furrows in it, so that these 2 trees seem to grow faster than they can grow well. In 1733, N° 23 was about as thick as a pitch-fork shaft. The elm N° 37 was planted with the quick in January, 1756, and cut down to the ground as that was. It is a kind of witch elm, which grow faster than the upright ones, and with large round heads. N° 38 is so far like a witch elm, that at 10 feet high it parts into a great head; but it grows much straighter and handsomer than that kind of tree generally does.

Planted trees at a distance from the hedge seem not to grow so large as sown trees in the hedge; whether from the check the roots receive in transplanting, or that the trees not in hedges are more rubbed by the cattle; perhaps both causes concur when the trees are transplanted large; but trees set in quicks, when very small, do not seem to be hurt by it. Mr. B. had some oaks set with the quick, and a row of acorns was some years after sown against it; but in between 40 and 50 years they have not overtaken the planted ones in size; the sown seem however inclined to be taller trees than the planted.

XXVIII. On the Era of the Mahometans, called the Hejerà. By William Marsden, Esq., F. R. S., and A. S.* p. 414.

In their computation of time, the Arabs, and other Mahometan nations, reckon by a year which is purely lunar. It has no reference to the solar revolutions, and is of course unconnected with the vicissitude of seasons. The purpose of its adoption appears to have been chiefly religious, for the regulation of fasts and ceremonies, rather than that of the civil concerns of the people. Perhaps a conscious ignorance in matters of science might have determined the institutors to prefer a period whose limits were marked and obvious to the senses, to one whose superior accuracy depended on astronomical calculation; and it may also be conjectured, that their habits of life rendered the adjustment of the tropical year less interesting to these turbulent and wandering fanatics, than to nations whose attention was directed to agriculture and other peaceful arts.

The era of the Mahometans, called by them the Hejerà, or Departure, is accounted from the year of the flight of Mahomet, their prophet, from Mecca, in Arabia Petræa, to Medina, at that time called Yatreb, which was the 13th of his pretended mission, the year of Christ 622, and of the Julian period 5335. This event, but little memorable in itself, and deriving no celebrity from

* As this mode of spelling the word differs from that commonly followed, it may be proper to observe, that the Arabic letters of which it is composed are h, j, r, á, or ah, and that the supplied vowels are to be pronounced short—Orig.

the circumstances immediately attending it, was, 18 years after, distinguished by the Caliph Omar, as the crisis of their new religion, and established as an epoch, to which the dates of all the transactions of the faithful should have reference in future. Before this, the people had been accustomed to compute from the commencement of a particular war, the day of a remarkable battle, or other occasional event of importance to their little communities. Accordingly, Mahomet is said to have been born in the first year of the era of the elephant, so called from an attack on the city and temple of Mecca, by a king of Abyssinian race, in which those animals were employed; and 20 years after this, the impious war, in which the animosity of two contending tribes occasioned them to violate the sacred or interdicted months, appeared of consequence sufficient to give rise to a new era. The uncertainty and confusion produced by this fluctuation demanded a reform, and more forcibly in proportion as the interests and concerns of the growing empire extended themselves. A dispute between two individuals, respecting the year in which the term of an obligation for money should be understood to expire, the parties being agreed as to the month, pointed out to the Caliph, to whose tribunal it was referred, the immediate necessity of enjoining the observance of a determinate era, in which the strongest prejudices of the people should be made to concur with the sovereign authority. The date of the Hejerà was thenceforth expressed in all the public acts and letters.

It must be understood, that though the account of the years, collectively considered, was vague, that of the months was certain, and their succession at all times scrupulously attended to. Omar did not think it expedient to attempt any innovation as to the time of beginning the year, against which the ideas of the people would have revolted; and therefore, though the escape of Mahomet from the indignation of his fellow citizens was effected, according to their records, on the first day of the 3d month, or rabee prior, on the 12th day of which he reached Medina, yet the Hejerà takes date from a period of 2 months antecedent to this flight, namely, from the first day of Moharram, being the day on which immemorial custom had established the celebration of the festival of the new year.

The Arabian and Syrian Christians, and the Mahometan astronomers in general, appear to have fixed this day to Thursday the 15th of the Syro-Macedonian month Tamooz, answering to our July; but some among the latter, and most of their historical writers, refer it to the next day, Friday the 16th, and this latter date has, in modern times, obtained almost universal acceptance. A religious preference which Friday claims above the rest of the week, seems to have given effect to the arguments in its favour. The difference of opinion on this subject has arisen, in the first place, from the uncertainty unavoidably

attending a date, to be ascertained, at a distant period of time, from the phase of the moon, which is retarded or advanced by so complicated a variety of circumstances: and the ambiguity appears, in the 2d place, to have been promoted by the custom of the Arabs beginning their day at sun-set; conformably with which idea, the time when the moon became visible at Mecca, being the evening of Thursday the 15th, according to our mode of computation, was to them the commencement of Friday; which Friday, beginning a few hours later, we term the 16th of July. At that period the cycle of the sun was 15; the cycle of the moon, or golden number, 15; the Roman indiction 10; and the dominical letter c.

The year of the Mahometans consists of 12 lunar months, and no embolism being employed to adjust it to the solar period (as practised by the Chaldæans and Hebrews, who were in other particulars their guides, and anciently, it is said, by the Arabs themselves), the commencement of each successive lunar year anticipates the completion of the solar, and revolves through all its seasons, the months respectively preserving no correspondence. In order to form a just and accurate idea of the length of this year, and of its component months, it will be necessary to distinguish 2 modes of estimating their commencement and duration. These, though their difference is not progressive, never amounting to more than 2 whole days, and rarely to so much as 1, may yet, if misunderstood, occasion in some instances, uncertainty and error: and more especially as the writers on this subject have inadvertently fallen into contradictions, from neglecting to explain to their readers a distinction of which they must have been themselves sufficiently aware. These modes may be denominated the vulgar or practical, and the political or chronological reckoning.

The vulgar or practical reckoning is that which estimates the commencement of the year, or first day of the month Moharram, from the appearance of the new moon, on the evening of the 1st or 2d day after the conjunction, or from that time at which it might from its age be visible, if not obscured by the circumstances of the weather, which is scarcely ever so soon as 24 hours, and seldom later than 48 hours, after the actual change. This appearance is announced by persons placed on the pinnacles of the mosques or other elevated situations, to the people below, who welcome it with the sound of instruments, firing of guns, and other demonstrations of respect and zeal. The month thus commenced is computed to last till the new moon again becomes visible; and so of the remaining months, till she has completed her 12th lunation, and, emerging from the sun's rays, marks the practical commencement of another year.

In the political or chronological mode of reckoning, the return of a new year, or the duration of the months which compose it, is not regulated either

by the appearance of the moon, or the calculated period of conjunction, but according to a certain division of a cycle of 30 years, adopted for this purpose.* Particular attention is due to the explanation of this mode, both as being more artificial and complex, and because it serves to regulate the dates in matters of historical record, and indeed of all writings where pretension is made to accuracy. On this the Turkish, Moorish, and every systematic Mahometan calendar are founded.

The lunar month, or mean synodic revolution, according to the computation of the Arabian astronomers, consists of 29 days, 12 hours, and 792 scruples or parts in 1080; and the year of 354 days, 8 hours, and 864 scruples. But, as the purposes of mankind require that the year should contain an integral number of days, it became expedient to collect and dispose of these fractional exceedings in a consistent and practical manner; and with this view, a cycle or period of 30 lunar years was chosen, as the lowest number that admitted of their being formed into days, without sensible deficiency or remainder. Their sum being 11 days, it was determined that 19 of those 30 years should be composed of 354 days, and 11 of 355 days each. The justness of this proportion will equally appear, if it be observed, that 8 hours and 864 scruples, or 48 minutes, constitute 11 parts in 30 of 24 hours, and consequently in 30 years produce an excess of 11 whole days.† It remained next to be considered in what order and method these additional or intercalary days should be inserted, so as to affect the compensation required with as much equability as possible, and maintain a correspondence, as near as circumstances would admit, with the periods marked by the phases of the moon. The following are the years to which, for reasons that shall be afterwards assigned, it was judged proper to annex an extraordinary day, and which, in contradistinction to those 19 that have only 354 days, are termed years of excess, viz. the 2d, 5th, 7th, 10th, 13th, 16th, 18th, 21st, 24th, 26th, and 29th, of the cycle of 30 years.

Their months, conformably with those of the Hebrew calendar, it was determined should consist alternately of 30 and 29 days; and therefore, in an ordi-

* A passage in Alfraganus (who wrote about the year of Christ 950) would lead us to infer, that besides the 2 ways of computing time here distinguished, the astronomers were accustomed to follow a 3d, whose periods were marked by the conjunction of the luminaries: but, as this learned Mahometan was a professed student of Ptolemy's works, which in this place he quotes, we may conclude that, when he speaks of astronomers, he does not mean to confine the expression to those of his own country or religion.—Orig.

† The mean synodic revolution being $29^d\ 12^h\ 44^m$ and nearly 3^s, this cycle falls short of 30 complete lunar years, by something more than 17^m, and consequently advances 1 day in about 2500 years. The Chaldæans, who made the time of the revolution to consist of 1 scruple, or 1080th part of an hour, more than the Arabs thought fit to allow, were wonderfully near to the truth. If, instead of 30 years, a cycle of 19 had been chosen, and 7 days intercalated, there would have been an excess of a 30th part of a day, which would have caused the reckoning to retrograde 1 day in 570 years.—Orig.

nary or simple year of 354 days, the 12th and last month, Dulhajee, would have only 29; but, in the years of excess, the intercalary day is added to this month, which is then made to consist of 30 days, and the year, consequently, of 355 days. Thus, for example, in the year of Christ 622, the Hejerà commenced on the 16th of July, with the Arabian month

	1st year.	2d year.
Moharram, which had days	30	30
Safar	29	29
Rabee prior	30	30
Rabee posterior	29	29
Joomad prior	30	30
Joomad posterior	29	29
Carried over	177	177
1st year ended 5 July 623.		

	1st year.	2d year.
Brought over	177	177
Rajab	30	30
Saban	29	29
Hamadan	30	30
Sawal	29	29
Dulkaidat.	30	30
Dulhajee	29	30
	354	355
2d year ended 25 June 624.		

It may not be uninteresting to examine the rule by which the Arabians appear to have been guided, in placing the intercalary day at the end of those particular years which have been specified. It was observed that the annual excess is calculated to be 11 parts in 30 of a day. At the commencement of the first year of their first cycle, they appear to have assumed the fact, somewhat capriciously, that there was an excess of 11 parts, belonging to the preceding year, to be accounted for, or brought on. At the end of the first year there would consequently be 22 such parts; and at the end of the 2d year 33 parts. Here then the first intercalary day was applied; that 2d year was made to consist of 355 days, and there remained 3 parts, over and above, to be carried on to the next. At the expiration of the 3d year, the parts amounted to 14: of the 4th year, to 25; and of the 5th, to 36; when the intercalation was again applied, and a balance of 6 parts carried on. From this it will be understood in what manner the fractional exceedings of each year were combined and disposed of through the succeeding years of the cycle; and it will be necessary only further to remark that, when the aggregate of the fractions falls short no more than 2 or 3 parts of the number of 30, they still add the intercalary day, and deduct the deficiency from the excess of the following year, which, in the course of 1 cycle, takes place only 3 times. At the end of the 29th year, the accumulated fractions, amounting exactly to 30, are commensurate with the intercalation then applied; and the excess of the 30th, or last year, is accounted for in the first intercalation of the succeeding period. The operation would doubtless have appeared more methodical, if the first intercalary day were not to have been added till the end of the 3d year, and the 11th, or last, till the end of the 30th year or termination of the cycle. From this consideration some commentators have been led to dissent from the more general idea, as above given, and to

suggest, that the embolism is in fact applied so soon after the commencement of the cycle, as the yearly accumulation of the fractional parts exceeds the sum of half a day, or 12 hours, and that it accordingly is made to take place at the end of the 2d year, because the fractions then amount to $17^h\ 36^m$, or 22 parts in 30; at the end of the 5th year, because they then amount to 25; and at the end of the 7th year, to 17 parts; keeping thus as near as possible to the mean division of time, by applying the compensation before it is fully wanted. The effect however is in both cases the same, and it is of but little moment to determine which theory is right. This cycle of 30 Mahometan years, contains 10,631 days, and is equal to 29 years and 39 days of our computation. The annual mean difference is 10 days and 21 hours nearly; which in common calculations, for short periods of time, may be reckoned at 11 days, by which number the lunar year anticipates the solar.

Annexed is a table exhibiting the correspondence of the years of the Hejerà, from the establishment of that epoch, with those of the Christian era, to the year of our Lord 2000. Until the beginning of the present century, it appears sufficient to distinguish every 10th year; the intervals between which may be calculated with ease and precision, by attending to what has been said respecting the cycle. From the year 1700 to the conclusion of the 20th century, for the convenience of historians yet unborn, the commencement of each year of the Hejerà is ascertained.

Table exhibiting the Correspondence of the Years of the Hejerà, with those of the Christian Era.

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
1	622	16 July	F	231	845	7 Sept.	M	461	1068	31 Oct.	F
11	632	29 Mar.	Su	241	855	22 May	W	471	1078	14 July	Sa.
21	641	10 Dec.	M	251	865	2 Feb.	F	481	1088	27 Mar.	M
31	651	24 Aug.	W	261	874	16 Oct.	Sa	491	1097	9 Dec.	W
41	661	7 May	F	271	884	29 June	M	501	1107	22 Aug.	Th
51	671	18 Jan.	Sa	281	894	13 March	W.	511	1117	5 May	Sa
61	680	1 Oct.	M	291	903	24 Nov.	Th	521	1127	17 Jan.	M
71	690	15 June	W	301	913	7 Aug.	Sa	531	1136	29 Sept.	Tu
81	700	26 Feb.	Th	311	923	21 April	M	541	1146	13 June	Th.
91	709	9 Nov.	Sa	321	933	1 Jan.	Tu	551	1156	25 Feb.	Sa
101	719	24 July	M	331	942	15 Sept.	Th	561	1165	7 Nov.	Su
111	729	5 April	Tu	341	952	29 May	Sa	571	1175	22 July	Tu.
121	738	18 Dec.	Th	351	962	9 Feb.	Su	581	1185	4 April	Th
131	748	31 Aug.	Sa	361	971	24 Oct.	Tu	591	1194	16 Dec.	F
141	758	14 May	Sa	371	981	7 July	Th	601	1204	29 Aug.	Su
151	768	26 Jan.	Tu	381	991	20 Mar.	F	611	1214	13 May	Tu.
161	777	9 Oct.	Th	391	1000	1 Dec.	Su	621	1224	24 Jan.	W
171	787	22 June	F	401	1010	15 Aug.	Tu	631	1233	7 Oct.	F.
181	797	5 March	Su	411	1020	27 April	W	641	1243	21 June	Su.
191	806	17 Nov.	Tu	421	1030	9 Jan.	F	651	1253	3 Mar.	M
201	816	30 July	W	431	1039	23 Sept.	Su	661	1262	15 Nov.	W
211	826	13 April	F	441	1049	5 June	M	671	1272	29 July	F
221	835	26 Dec.	Su	451	1059	17 Feb.	W	681	1282	11 April	Sa.

An. Hej.	An. D.		Day.
691	1291	24 Dec.	M
701	1301	6 Sept.	W
711	1311	20 May	Th
721	1321	31 Jan.	Sa
731	1330	15 Oct.	M
741	1340	27 June	Tu
751	1350	11 Mar.	Th
761	1359	23 Nov.	Sa
771	1369	5 Aug.	Su
781	1379	19 April	Tu
791	1388	31 Dec.	Th
801	1398	13 Sept.	F
811	1408	27 May	Su
821	1418	8 Feb.	Tu
831	1427	22 Oct.	W
841	1437	5 July	F
851	1447	19 Mar.	Su
861	1456	3 Nov.	M
871	1466	13 Aug.	W
881	1476	26 April	F
891	1486	7 Jan.	Sa
901	1495	21 Sept.	M
911	1505	4 June	W
921	1515	15 Feb.	Th
931	1524	29 Oct.	Sa
941	1534	13 July	M
951	1544	25 March	Tu
961	1553	7 Dec.	Th
971	1563	21 Aug.	Sa
981	1573	3 May	Su
991	1583	15 Jan.	Tu
1001	1592	28 Sept.	Th
1011	1602	11 June	F
1021	1612	23 Feb.	Su
1031	1621	6 Nov.	Tu
1041	1631	20 July	W
1051	1641	2 April	F
1061	1650	15 Dec.	Su
1071	1660	27 Aug.	M
1081	1670	11 May	W
1091	1680	23 Jan.	F
1101	1689	5 Oct.	Sa
1111	1699	19 June	M
1112	1700	7 June	F
1113	1701	28 May	W
1114	1702	17 May	Su
1115	1703	6 May	Th
1116	1704	25 April	Tu
1117	1705	14 April	Sa
1118	1706	4 April	Th
1119	1707	24 March	M
1120	1708	12 March	F
1121	1709	2 March	W
1122	1710	19 Feb.	Su
1123	1711	8 Feb.	Th
1124	1712	29 Jan.	Tu
1125	1713	17 Jan.	Sa
1126	1714	7 Jan.	Th
1127	1714	27 Dec.	M
1128	1715	16 Dec.	F
1129	1716	5 Dec.	W
1130	1717	24 Nov.	Su
1131	1718	13 Nov.	Th
1132	1719	3 Nov.	Tu
1133	1720	22 Oct.	Sa
1134	1721	11 Oct.	W
1135	1722	1 Oct.	M
1136	1723	20 Sept.	F
1137	1724	9 Sept.	W
1138	1725	29 Aug.	Su
1139	1726	18 Aug.	Th
1140	1727	8 Aug.	Tu
1141	1728	27 July	Sa
1142	1729	16 July	W
1143	1730	6 July	M
1144	1731	25 June	F
1145	1732	13 June	Tu
1146	1733	3 June	Su
1147	1734	23 May	Th
1148	1735	13 May	Tu
1149	1736	1 May	Sa
1150	1737	20 April	W
1151	1738	10 April	M
1152	1739	30 March	F
1153	1740	18 March	Tu
1154	1741	8 March	Su
1155	1742	25 Feb.	Th
1156	1743	15 Feb.	Tu
1157	1744	4 Feb.	Sa
1158	1745	23 Jan.	W
1159	1746	13 Jan.	M
1160	1747	2 Jan.	F
1161	1747	22 Dec.	Tu
1162	1748	11 Dec.	Su
1163	1749	30 Nov.	Th
1164	1750	19 Nov.	M
1165	1751	9 Nov.	Sa
1166	1752	8 Nov.	W
1167	1753	29 Oct.	M
1168	1754	18 Oct.	F
1169	1755	7 Oct.	Tu
1170	1756	26 Sept.	Su
1171	1757	15 Sept.	Th
1172	1758	4 Sept.	M
1173	1759	25 Aug.	Sa
1174	1760	13 Aug.	W
1175	1761	2 Aug.	Su
1176	1762	23 July	F
1177	1763	12 July	Tu
1178	1764	1 July	Su
1179	1765	20 June	Th
1180	1766	9 June	M
1181	1767	30 May	Sa
1182	1768	18 May	W
1183	1769	7 May	Su
1184	1770	27 April	F
1185	1771	16 April	Tu
1186	1772	5 April	Su
1187	1773	25 March	Th
1188	1774	14 March	M
1189	1775	4 March	Sa
1190	1776	21 Feb.	W
1191	1777	9 Feb.	M
1192	1778	30 Jan.	F
1193	1779	19 Jan.	Tu
1194	1780	8 Jan.	Su
1195	1780	28 Dec.	Th
1196	1781	17 Dec.	M
1197	1782	7 Dec.	Sa
1198	1783	26 Nov.	W
1199	1784	14 Nov.	Su
1200	1785	4 Nov.	F
1201	1786	24 Oct.	Tu
1202	1787	13 Oct.	Sa
1203	1788	2 Oct.	Th
1204	1789	21 Sept.	M
1205	1790	10 Sept.	F
1206	1791	31 Aug.	W
1207	1792	19 Aug.	Su
1208	1793	9 Aug.	F
1209	1794	29 July	Tu
1210	1795	18 July	Sa
1211	1796	7 July	Th
1212	1797	26 June	M
1213	1798	15 June	F
1214	1799	5 June	W
1215	1800	24 May	Su
1216	1801	14 May	F
1217	1802	3 May	Tu
1218	1803	22 April	M
1219	1804	11 April	Th
1220	1805	31 March	M
1221	1806	20 March	F
1222	1807	10 March	W
1223	1808	27 Feb.	Su
1224	1809	15 Feb.	Th
1225	1810	5 Feb.	Tu
1226	1811	25 Jan.	Sa
1227	1812	15 Jan.	Th
1228	1813	3 Jan.	M
1229	1813	23 Dec.	F
1230	1814	13 Dec.	W
1231	1815	2 Dec.	Su
1232	1816	20 Nov.	Th
1233	1817	10 Nov.	Tu
1234	1818	30 Oct.	Sa
1235	1819	19 Oct.	W
1236	1820	8 Oct.	M
1237	1821	27 Sept.	F
1238	1822	17 Sept.	W

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
1239	1823	6 Sept.	Su	1296	1878	23 Dec.	Th	1353	1934	14 April	M
1240	1824	25 Aug.	Th	1297	1879	14 Dec.	M	1354	1935	3 April	F
1241	1825	15 Aug.	Tu	1298	1880	3 Dec.	Sa	1355	1936	22 March	Tu
1242	1826	4 Aug.	Sa	1299	1881	22 Nov.	W	1356	1937	12 March	Su
1243	1827	24 July	W	1300	1882	11 Nov.	Su	1357	1938	1 March	Th
1244	1828	13 July	M	1301	1883	1 Nov.	F	1358	1939	19 Feb.	Tu
1245	1829	2 July	F	1302	1884	20 Oct.	Tu	1359	1940	8 Feb.	Sa
1246	1830	22 June	W	1303	1885	9 Oct.	Sa	1360	1941	27 Jan.	W
1247	1831	11 June	Su	1304	1886	29 Sept.	Th	1361	1942	17 Jan.	M
1248	1832	30 May	Th	1305	1887	18 Sept.	M	1362	1943	6 Jan.	F
1249	1833	20 May	Tu	1306	1888	7 Sept.	Sa	1363	1943	26 Dec.	Tu
1250	1834	9 May	Sa	1307	1889	27 Aug.	W	1364	1944	15 Dec.	Su
1251	1835	28 April	W	1308	1890	16 Aug.	Su	1365	1945	4 Dec.	Th
1252	1836	17 April	M	1309	1891	6 Aug.	F	1366	1946	24 Nov.	Tu
1253	1837	6 April	F	1310	1892	25 July	Tu	1367	1947	13 Nov.	Sa
1254	1838	26 March	Tu	1311	1893	14 July	Sa	1368	1948	1 Nov.	W
1255	1839	16 March	Su	1312	1894	4 July	Th	1369	1949	22 Oct.	M
1256	1840	4 March	Th	1313	1895	23 June	M	1370	1950	11 Oct.	F
1257	1841	22 Feb.	Tu	1314	1896	11 June	F	1371	1951	30 Sept.	Tu
1258	1842	11 Feb.	Sa	1315	1897	1 June	W	1372	1952	19 Sept.	Su
1259	1843	31 Jan.	W	1316	1898	21 May	Su	1373	1953	8 Sept.	Th
1260	1844	21 Jan.	M	1317	1899	11 May	F	1374	1954	23 Aug.	M
1261	1845	9 Jan.	F	1318	1900	29 April	Tu	1375	1955	18 Aug.	Sa
1262	1845	29 Dec.	Tu	1819	1901	18 April	Sa	1376	1956	6 Aug.	W
1263	1846	19 Dec.	Su	1320	1902	8 April	Th	1377	1957	27 July	M
1264	1847	8 Dec.	Th	1321	1903	28 March	M	1378	1958	16 July	F
1265	1848	26 Nov.	M	1322	1904	16 March	F	1379	1959	5 July	Tu
1266	1849	16 Nov.	Sa	1323	1905	6 March	W	1380	1960	24 June	Su
1267	1850	5 Nov.	W	1324	1906	23 Feb.	Su	1381	1961	13 June	Th
1268	1851	26 Oct.	M	1325	1907	12 Feb.	Th	1382	1962	2 June	M
1269	1852	14 Oct.	F	1326	1908	2 Feb.	Tu	1383	1963	23 May	Sa
1270	1853	3 Oct.	Tu	1327	1909	21 Jan.	Sa	1384	1964	11 May	W
1271	1854	23 Sept.	Su	1328	1910	11 Jan.	Th	1385	1965	30 April	Su
1272	1855	12 Sept.	Th	1329	1910	31 Dec.	M	1386	1966	20 April	F
1273	1856	31 Aug.	M	1330	1911	20 Dec.	F	1387	1967	9 April	Tu
1274	1857	21 Aug.	Sa	1331	1912	9 Dec.	W	1388	1968	29 March	Su
1275	1858	10 Aug.	W	1332	1913	28 Nov.	Su	1389	1969	18 March	Th
1276	1859	31 July	M	1333	1914	17 Nov.	Th	1390	1970	7 March	M
1277	1860	19 July	F	1334	1915	7 Nov.	Tu	1391	1971	25 Feb.	Sa
1278	1861	8 July	Tu	1335	1916	26 Oct.	Sa	1392	1972	14 Feb.	W
1279	1862	28 June	Su	1336	1917	16 Oct.	Th	1393	1973	2 Feb.	Su
1280	1863	17 June	Th	1337	1918	5 Oct.	M	1394	1974	23 Jan.	F
1281	1864	5 June	M	1338	1919	24 Sept.	F	1395	1975	12 Jan.	Tu
1282	1865	26 May	Sa	1339	1920	13 Sept.	W	1396	1976	2 Jan.	Su
1283	1866	15 May	W	1340	1921	2 Sept.	Su	1397	1976	21 Dec.	Th
1284	1867	4 May	Su	1341	1922	22 Aug.	Th	1398	1977	10 Dec.	M
1285	1868	23 April	F	1342	1923	12 Aug.	Tu	1399	1978	30 Nov.	Sa
1286	1869	12 April	Tu	1343	1924	31 July	Sa	1400	1979	19 Nov.	W
1287	1870	2 April	Su	1344	1925	20 July	W	1401	1980	7 Nov.	Su
1288	1871	22 March	Th	1345	1926	10 July	M	1402	1981	28 Oct.	F
1289	1872	10 March	M	1346	1927	29 June	F	1403	1982	17 Oct.	Tu
1290	1873	28 Feb.	Sa	1347	1928	18 June	W	1404	1983	6 Oct.	Sa
1291	1874	17 Feb.	W	1348	1929	7 June	Su	1405	1984	25 Sept.	Th
1292	1875	6 Feb.	Su	1349	1930	27 May	Th	1406	1985	14 Sept.	M
1293	1876	27 Jan.	F	1350	1931	17 May	Tu	1407	1986	4 Sept.	Sa
1294	1877	15 Jan.	Tu	1351	1932	5 May	Sa	1408	1987	24 Aug.	W
1295	1878	4 Jan.	Sa	1352	1933	24 April	W	1409	1988	12 Aug.	Su

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
1410	1989	2 Aug.	F	1414	1993	19 June	M	1418	1997	7 May	F
1411	1990	22 July	Tu	1415	1994	8 June	F	1419	1998	26 April	Tu
1412	1991	11 July	Sa	1416	1995	29 May	W	1420	1999	15 April	Sa
1413	1992	30 June	Th	1417	1996	17 May	Su	1421	2000	4 April	Th

END OF THE SEVENTY-EIGHTH VOLUME OF THE ORIGINAL.

I. Description of an Improvement in the Application of the Quadrant of Altitude to a Celestial Globe, for the Resolution of Problems dependant on Azimuth and Altitude. By Mr. John Smeaton, F. R. S. Anno 1789. *Vol. LXXIX.* p. 1.

The difficulty that has occurred in fixing a semi-circle, so as to have a centre in the zenith and nadir points of the globe, at the same time that the meridian is left at liberty to raise the pole to its desired elevation, Mr. S. supposes, has induced the globe-makers to be contented with the strip of thin flexible brass, called the quadrant of altitude; and it is well known how imperfectly it performs its office. The improvement he has attempted, is in the application of a quadrant of altitude, of a more solid construction; which being affixed to a brass socket of some length, and this ground and made to turn on an upright steel spindle, fixed in the zenith, steadily directs the quadrant, or rather arc, of altitude to its true azimuth, without being at liberty to deviate from a vertical circle to the right hand or left: by which means the azimuth and altitude are given with the same exactness as the measure of any other of the great circles.

With respect to the horary circle, as the common application seems very convenient on account of the ready adjustment of its index to answer the culmination of any of the heavenly bodies; and as a circle of 4 inches diameter is capable of an actual and very distinguishable division into 720 parts, answerable to 2 minutes of time each, which may serve a globe of the largest size; it seems that it should rather be improved than omitted; and if, instead of a pointer, an index stroke is used in the same plane with that of the divisions, the single minutes, and even half minutes, may be readily distinguished. This globe, though mounted merely as a model for experiment, and only 9 inches in diameter, appears capable of bringing out the solution to a quarter of a degree; which may be esteemed sufficient, not only as a check on numerical computation, but to come near enough to find stars in the day-time in the field of telescopes, which, having no equatorial motion, are only capable of direction in altitude and azimuth; but from globes of a larger size, we may expect to come proportionably nearer.

Mr. S. adds a minute description of the several new parts of this globe, which is illustrated with a large plate of the same; not necessary to be reprinted here.

II. Objections to the Experiments and Observations relating to the Principle of Acidity, the Composition of Water, and Phlogiston, considered; with further Experiments and Observations on the same Subject. By the Rev. J. Priestley, LL. D., F. R. S. p. 7.

Having never failed, says Dr. P., when the experiments were conducted with due attention, to procure some acid whenever I decomposed dephlogisticated and inflammable air in close vessels, I concluded that an acid was the necessary result of the union of those 2 kinds of air, and not water only; which is an hypothesis that has been maintained by Mr. Lavoisier and others, and which has been made the basis of an entirely new system of chemistry, to which a new system of terms and characters has been adapted. The facts that I alleged were not disputed; but to my conclusion it was objected, that the acid I procured might come from the phlogisticated air, which in one of my processes could not be excluded; and that it was reasonable to conclude that this was the case, because Mr. Cavendish had procured the same acid, viz. the nitrous, by decomposing dephlogisticated and phlogisticated air with the electric spark. In other cases it has been said, that the fixed air I procured came from the plumbago in the iron from which my inflammable air had been extracted.

With respect to the former of these objections I would observe, that my process is very different from that of Mr. Cavendish; his decomposition being a very slow one by electricity, and mine a very rapid one by simple ignition, a process by which phlogisticated air, as I found by actual trial, was not at all affected; the dephlogisticated and inflammable airs uniting, and leaving the phlogisticated air (as they probably would any other kind of air with which they might have been mixed) just as it was. I would also observe, that there is no contradiction whatever between Mr. Cavendish's experiment and mine, since phlogisticated air may contain phlogiston, and by means of electricity this principle may be evolved, and unite with the dephlogisticated air, or with the acid principle contained in it, as in the process of simple ignition the same principle is evolved from inflammable air, in order to form the same union; in consequence of which, the water, which was a necessary ingredient in the composition of both the kinds of air, is precipitated. That in other circumstances than those in which I made the experiments, the acid wholly escaped, and nothing but water was found, may be easily accounted for, from the small quantity of the acid principle in proportion to the water, and the extreme volatility of it, owing, I presume, to its high phlogistication when formed in this manner.

In order to ascertain the effect of the presence of phlogisticated air in this process, I now not only repeated the experiment of mixing a given quantity of phlogisticated air with the 2 other kinds of air, and found, as before, that it was not affected by the operation; but I made the experiment with atmospheric air, instead of dephlogisticated. Since the air of the atmosphere contains a

greater proportion of phlogisticated air, it might be expected that, if the acid I got before came from the small quantity of phlogisticated air which I could not possibly exclude, I should certainly get more acid when, instead of endeavouring to exclude it, I purposely introduced a greater quantity. But the consequence was the production of much less acid than before, the liquor I procured being sometimes not to be distinguished from pure water, except by the greatest attention possible: for though the decomposition was made in the same copper vessel which I used in the former experiments, there was now no sensible tinge of green colour in it.

When I repeated this experiment in a glass vessel, I perceived, as I imagined, the reason of the small produce of acid in these new circumstances: for the vessel was filled with a vapour which was not soon condensed, and being diffused through the phlogisticated air, (which is not affected by the process) is drawn away along with it, when the exhausting of the tube is repeated; whereas, when there is little or no air in the vessel besides the 2 kinds that unite with each other, and are decomposed, the acid vapour, having nothing to attach itself to and support it (by being entangled with it) much sooner attacks the copper, making the deep green liquor which I have described. Sometimes however I have procured a liquor which was sensibly green by the decomposition of atmospheric and inflammable air, but by no means of so deep a colour, or so sensibly acid, as when the dephlogisticated air is used.

The extreme volatility of the acid thus formed (and which accounts for the escape of some part of it in all these processes) is apparent from this circumstance, that if the explosions be made in quick succession (the tube being exhausted immediately after each of them, and filled again as soon as possible) no liquor at all will be collected, the whole of the acid vapour, together with the water with which it was combined, being drawn off uncondensed in every process. I once made 20 successive explosions of this kind, in a copper tube, out of which I found that I drew 37 oz. measures of air by the action of the pump, and found not a single drop of liquid, though near an hour was employed in the whole process, and the vessel was never made more than a little warmer than my hand. This was a degree of heat by no mean sufficient to keep the whole of any quantity of water in a state of vapour; and is a circumstance which of itself sufficiently proves, that the vapour did not consist of water only.

Indeed, I think it impossible for any one to see this vapour in a tall glass vessel, and especially to observe how it falls from one end of it to the other, and the time that is required to its wholly disappearing, without being satisfied that it consists of something else than mere water, the vapour of which would be more equally diffused. If the appearance to the eye should fail to convince any person of this, the sense of smell would do it: for even in a glass vessel it is

very offensive, though it might not be pronounced to be acid. I conjecture however that this, and every other species of smell, is produced by some modification of the acid or alkaline principle. Some may be disposed to ascribe this smell to the iron from which the inflammable air was produced; but the smell is the same, or nearly so, when the air is from tin, and would probably be the same if it were from any other substance. Besides using atmospheric air, which contains a greater proportion of phlogisticated air, I have sometimes used dephlogisticated air which was not very pure; and in this case I have always observed, that the liquor I procured had less colour, and was less sensibly acid. These observations might, I should think, satisfy any reasonable person, that the acid liquor which I procured by the explosion of dephlogisticated and inflammable air in close vessels did not come from the phlogisticated air which could not be excluded, whether it was that which remained in the vessel after exhausting it by the air-pump, or that with which the dephlogisticated air was more or less contaminated.

But besides these experiments, in which I procured the green acid liquor by the explosion of dephlogisticated and inflammable air in close vessels, I made another, to which I thought the same objection could not have been made, because no air-pump was used in it, and nothing but the purest dephlogisticated air was employed, being separated in the process from precipitate per se in contact with the purest inflammable air in a glass vessel which had been previously filled with mercury. Accordingly, the only objection made to this experiment was, that the preparation used might be impure, containing something which might yield phlogisticated air. This appeared to me highly improbable, as the precipitate had been made by M. Cadet, and for the purpose of philosophical experiments. Besides, if the heat of a burning lens should dislodge phlogisticated air from any unperceived impurity in this preparation, mere heat will not decompose this air. Let any person try the effect of a lens on such air, or any substance containing it, and produce an acid if he can.

M. Berthollet however, thinking that this might be the case, desired that I would send him a specimen of my precipitate per se. Accordingly, I sent him all that remained of it; and in return he sent me a quantity on the goodness of which I might depend. With this preparation I repeated my former experiment; and, by giving more attention to the process, found it to be far more decisively conclusive in favour of my opinion than I had imagined. In the former experiment I had attended only to the drop of water which was found in the vessel in which the process was made; and finding that it turned the juice of turnsole red, I concluded that it contained nitrous acid: but I now examined the air that remained in the vessel, and found that a considerable proportion of it was fixed air; so that I am now satisfied this was the acid with which it was

impregnated, and not the nitrous. Still however some acid is the constant result of the union of the two kinds of air, and not water only. A quantity of the same precipitate per se yielded no fixed air by heat. Comparing this experiment with that in which iron is ignited in dephlogisticated air, this general conclusion may be drawn, viz. that when either inflammable or dephlogisticated air is extracted from any substance in contact with the other kind of air, so that one of them is made to unite with the other in what may be called its nascent state, the result will be fixed air; but that if both of them be completely formed before their union, the result will be nitrous acid.

It has been said, that the fixed air produced in both these experiments may come from the plumbago in the iron from which the inflammable air is obtained. But since we ascertain the quantity of plumbago contained in iron by what remains after its solution in acids, it is in the highest degree improbable, that whatever plumbago there may be in iron, any part of it should enter into the inflammable air procured from it. Besides, according to the antiphlogistic hypothesis, all inflammable air comes from water only. As it cannot be said, that any real fixed air is found in inflammable air from iron (since it is not discoverable by lime-water) it must be supposed, that the elements, or component parts of fixed air are in it; but one of these elements is pure air, and the mixture of nitrous air shows that it contains no such thing, though, according to M. Lavoisier, fixed air contains 72 parts in 100 of pure air.

However, being apprized of this objection to inflammable air from iron, I made use of inflammable air from tin, and I had the same result as with that from iron. I also calculated the weight of the fixed air which I got in the process, and comparing it with the plumbago which the iron necessary to make the inflammable could have contained, I found that in all the cases it far exceeded the weight of the plumbago; so that it was absolutely impossible, that the fixed air which I found should have had this origin. For the greater satisfaction, Dr. P. recites the particulars of a few experiments of this kind. But, for these, we must refer to a separate pamphlet which Dr. P. published on this subject.

III. Observations on the Class of Animals called, by Linnæus, Amphibia; particularly on the Means of Distinguishing those Serpents which are Venomous, from those which are not so. By Edward Whitaker Gray, M. D., F. R. S. p. 21.

Of the various classes of the animal kingdom, says Dr. G., no one has been so little attended to as the class, called by Linnæus, Amphibia. What he himself did in that class, though far superior to what any other person has done, was evidently done in a hurry; false references are at least as common in that as in any other part of his works, and many of his descriptions are given in a very

careless manner; there are others however which are truly worthy of their author, and in which the specific characters are pointed out with that clearness and precision, which so eminently distinguish the descriptions of Linnæus from those of all his predecessors.

In the construction of the class, Linnæus has been particularly unfortunate; as he has erred, not only in making a unilocular heart one of the characters of it, but also in making the cartilaginous fishes a part of it. Every anatomist now agrees that the Amphibia Nantes are not furnished with lungs; and every naturalist is convinced of the propriety of removing them from the class of amphibia, to that of fishes. By the removal, the name of the class becomes much less objectionable; there being few genera, in the 2 orders of which it is now presumed to consist, which do not contain animals to which the term amphibious may, with some propriety, be given; whereas, in the order of Nantes, not one species occurs which has the smallest claim to that title. With respect to the other error, viz. that of supposing the hearts of the amphibia to be single, it would be easy to show that it was not an uncommon one, at the time Linnæus formed his system. And indeed it appears he was led into it, by following an author whom he probably supposed of too great fame not to be safely relied on. At least, in defence of his opinion, he quotes the following words of Boerhaave. "In omnibus animalibus in quibus sanguis non calet, ventriculus cordis est unicus." It is sufficient for the purpose to observe, that the hearts of most of the amphibia are now well known to be double, with an immediate communication between the 2 cavities; which structure seems peculiarly adapted to that change of element, which many of them can for a time support; and thereby furnishes another argument in favour of the name Linnæus has given to the class. To consider the structure of the heart however is not absolutely necessary in forming the characters of the class: the animals of which it consists being sufficiently distinguished from all others, by having cold red blood, and breathing by means of lungs. These 2 characters render the class perfectly distinct from the rest; the 2 superior ones, viz. mammalia and birds, having warm blood; and the 3 inferior ones, fishes, insects, and worms, not being furnished with lungs.

In his generic characters, Linnæus has been more successful than in those of the class. Whoever will be at the pains of comparing Linnæus's genera of amphibia with those of Gronovius, will find, that the generic characters of the former, though few in number, are precise and distinct; while those of the latter, though more numerous, are vague, indistinct, and sometimes inaccurate. But though Linnæus's genera of amphibia are generally well-formed, it must be allowed to be a great imperfection in them, that the venomous serpents are not separated from the others. From some expressions in the Preface to the Mu-

seum Regis, and in the introduction to the class amphibia, in the Systema Naturæ, it seems that he thought it not easy to distinguish them by any external characters; and his ideas respecting the venomous fangs themselves were so vague and confused, that it was hardly possible for him to attempt to found a generic distinction on them.

Whether venomous serpents can be with certainty distinguished from others, and if so, how they are to be known, is what is meant to be considered in this paper; in doing which Dr. G. examines, first, how far they may be distinguished by any external characters; 2dly, supposing the venomous fangs to be the only certain criterion, how those fangs are to be distinguished from common teeth. Though serpents, by their internal organization, naturally belong to the 3d class of the animal kingdom, they are in their external form more simple than most of the animals belonging to the 3 inferior classes; their external characters must consequently be very few.

In the 1st genus, crotalus, the head is broader than the neck, depressed or flat at top, and covered with small scales. These 3 characters are particularly observable in the 3 intermediate species horridus, dryinas, and durissus. In the miliarius the scales of the head are rather larger than in the others. The mutus certainly should not be placed among the crotali. As all the species of this genus are venomous, one is naturally led, by the examination of it, to consider the fore-mentioned characters as being, in some measure, proper to venomous serpents. In the genus coluber are many venomous species, and it is very certain that in general they have the fore-mentioned characters; examples of which may be seen in the atropos, cerastes, atrox, berus, and others. It is however equally certain that there are some in which they are not to be found. For example the naja, a species well known to be very venomous; the head of which is neither depressed nor broad, is covered with large scales, and is in every respect a complete exception to what has been said respecting the heads of venomous serpents. Since then there are venomous serpents in which the fore-mentioned characters, viz. a broad and depressed head, covered with small scales, are not to be found; let us examine whether those characters are to be found in any of those serpents which are not venomous. In the genus coluber there are very few, except venomous ones, which have the head much broader than the neck; and of those few every one has the head covered with large scales. But in the genus boa, though no species is venomous, except the contortrix, almost every one has the head broad, depressed, and covered with small scales. The canina, constrictor, hortulana, besides some others not described by Linnæus, furnish examples of this.

In the crotali Dr. G. has never found the tail, exclusive of the rattle, to exceed $\frac{1}{7}$ part of the whole length; sometimes it is much shorter. In some of

the venomous colubri, the proportion is still less. In the atropos it is found only $\frac{1}{11}$. In the English viper, coluber berus, it is commonly about $\frac{1}{7}$ or $\frac{1}{8}$. In some venomous species however the proportion is something greater. In the naja it is sometimes $\frac{1}{5}$; which proportion is, he thinks, as great as he has ever observed: at any rate he never met with a venomous serpent the tail of which was equal to $\frac{1}{4}$ of the whole length. With respect to those colubri which are not venomous, there are many whose tails are within the limits assigned to the venomous ones. In the coluber Æsculapii, doliatus, getulus, and some others, the tail is not generally more than $\frac{1}{7}$ of the whole length. In the lemniscatus it sometimes does not exceed $\frac{1}{18}$ or $\frac{1}{17}$, but perhaps in no other Linnæan species it is so short. In the greater number however the proportion of tail is more considerable; in many it is full $\frac{1}{4}$. In the ahætulla, and in some species not described by Linnæus, it is sometimes more than $\frac{2}{5}$; but never quite so long as the trunk, or half of the whole length. None of the Linnæan species, of the Boæ, have their tails either remarkably long, or short; but in 2 species, not described by Linnæus, Dr. G. has found the tail very little exceeding the proportion here assigned to the coluber lemniscatus. In the thickness of the tail, or in the acuteness of its termination, he has observed no difference worth remarking. In every species of the first 3 genera, the tail is thinner than the trunk; and in most of them it is more or less acute. The few exceptions he has observed were none of them venomous; but they are too few to deserve any particular consideration.

A character of great use in distinguishing the species of serpents, and which was not overlooked by Linnæus, is that elevated line, or carina, with which the scales of many species are furnished. In order to show how far this is to be considered as serving to distinguish venomous serpents from others, Dr. G. has examined 112 species of serpents, not venomous, belonging to the first 3 genera; and found that 80 of them have smooth scales, and 32 only have carinated ones. Of venomous serpents he has examined 26; of which number, 20 have carinated scales, and only 6 have smooth ones. On the whole therefore, carinated scales must be considered as being, in some measure, a character of venomous serpents.

The other 3 genera, anguis, amphisbæna, and cæcilia, besides the characters assigned them by Linnæus, have some others which are common to all, and which render them very different, in their external appearance, from any of the first 3 genera. These are, a very thick and obtuse tail, and a head which is very indistinct, and furnished with very small eyes. This last character is sometimes, though very rarely, met with among the colubri, for instance, in the lemniscatus; in the last 3 genera however it takes place without exception. The thickness of the tail is also common to every species; and though in the

anguis bipes, and in another species, not described by Linnæus, but figured in Browne's History of Jamaica (Tab. 44, fig. 1,) the tail has an acute termination, yet in both these species, especially in the last, it continues thick to the end, and becomes suddenly sharp, being what in botanical language would be called, obtusa cum acumine. With respect to the proportionate length of tail however, it is very remarkable that the genus anguis affords examples of much less proportion, and also of much greater, than is to be found in any of the first 3 genera. In the anguis scytale the tail is not above $\frac{1}{30}$ of the whole length; in the maculata it is not above $\frac{1}{40}$; yet in the anguis fragilis, and in the ventralis, the tail is always longer than the trunk, or is more than half the whole length. Indeed, in one specimen of the last-mentioned species, Dr. G. found the tail nearly $\frac{3}{5}$ of the whole length. It may however be questioned whether that species is really an anguis, or a lacerta.

The principal inferences to be deduced from those remarks, are the following: 1st. That a broad head, covered with small scales, though it be not a certain criterion of venomous serpents, is, with some few exceptions, a general character of them. 2dly, That a tail under $\frac{1}{5}$ of the whole length is also a general character of venomous serpents; but since many of those which are not venomous have tails as short, little dependance can be placed on that circumstance alone. On the other hand, a tail exceeding that proportion is a pretty certain mark that the species, to which it belongs, is not venomous. 3dly. That a thin and acute tail is by no means to be considered as peculiar to venomous serpents; though a thick and obtuse one is only to be found among those which are not venomous. 4thly. That carinated scales are, in some measure, characteristic of venomous serpents, since in them they are more common than smooth ones, in the proportion of nearly 4 to 1; whereas smooth scales are, in those serpents which are not venomous, more common, in the proportion of nearly 3 to 1.

On the whole therefore it appears, that though a pretty certain conjecture may, in many instances, be made from the external characters; yet, in order to determine with certainty whether a serpent be venomous or not, it becomes necessary to have recourse to some more certain diagnostic. This can only be sought for in the mouth. To those who form their ideas of the fangs of venomous serpents from those of the rattle-snake, or even from those of the English viper, it will appear strange that there should be any difficulty in distinguishing those weapons from common teeth; and indeed the distinction would really be very easy were all venomous serpents furnished with fangs as large as those of the fore-mentioned species. But the fact is, that in many species the fangs are full as small as common teeth, and consequently cannot, by their size, be known from them; this is the case with the coluber laticaudatus, lacteus, and several others.

Linnæus thought the fangs might be distinguished by their mobility; this, at least, may be fairly inferred, from his never mentioning them in the Museum Regis, without adding the epithet mobilia, except in one instance, the coluber aulicus. But with regard to mobility, considered in general as a character of venomous fangs, Dr. G. has not only never found it so, but he has also never been able to discover in them any thing which could properly be called mobility. He has indeed sometimes found some of them loose in their sockets; but then he has found others, in the same specimen, quite fixed. The same thing was observed both by Dr. Nicholls, and by the Abbé Fontana, in the common viper, even during life. The loose fangs may be such as have not yet been firmly fixed in their socket, or they may have been loosened by some accident: for the fangs may be at any time loosened, and even displaced, by a small degree of violence; which perhaps may be one reason why there is always a certain number of small fangs, near the base of the full grown ones, ready to enlarge and take their place, if they should be by any accident torn out.

Linnæus seems also to have thought that the fangs might be known by their situation. In the Introduction to the class Amphibia in the Systema Naturæ, he says they are, " Dentibus simillima sed extra maxillam superiorem collocata;" and in the description of the crotalus dryinas, in the Amœnitates Academicæ, he says, " Dentes ejus duo canini uti in reliquis venenatis serpentibus non in maxillis hærent, iis enim vulnerando, non autem ictus infligendo utitur." These 2 quotations show that Linnæus thought the situation of the fangs different from that of the common teeth; the last also shows that he thought their mode of action influenced by it. But the most singular opinion of Linnæus, respecting the venomous fangs, was, that they were sometimes fixed in the base of the jaw. Of this he has given 2 instances in the Museum Regis. One in the description of the coluber severus, of which he says, " Hastæ mobiles solitariæ versus basin maxillarum interius adhærent." The other in that of the coluber stolatus. His words there are, " Tela mobilia ad basin maxillarum affixa, ut vix vulnerare valeat hostes, solum cibos veneno inficere."

With respect to their size, it has already been observed that it is very various, consequently no certain judgment can, in all cases, be made from that circumstance. In some species they are so large, that their size alone sufficiently distinguishes them from common teeth; but in others they are so small, that it is very difficult to discover them. The size of the common teeth also varies very much, in different species. In the coluber mycterizans they are remarkably large, especially those which are situated near the apex of the upper jaw; which circumstance probably helped to lead Linnæus into the erroneous opinion he entertained, that this serpent was venomous. But in many species the teeth are so small, that it is impossible to discover, merely by looking into the mouth, that

the animal has any. Yet in that case they may be very easily detected, by drawing a pin, or any other hard substance, with a moderate degree of pressure, along the edge of the jaw, from the apex to the angle of the mouth, when they will be felt to grate against the pin, like the teeth of a saw.

Though the size of the venomous fangs is very various, their situation is always the same; namely, in the anterior and exterior part of the upper jaw, which situation he considers as the only one in which venomous fangs are ever found. But as, in those serpents which are not venomous, common teeth are found in that part of the jaw, it is plain that we cannot, by situation alone, distinguish one from the other. They may however be easily distinguished, and with great certainty, by the following simple operation. When it is discovered that there is something like teeth in the fore-mentioned part of the upper jaw, let a pin be drawn, in the manner described, from that part of the jaw to the angle of the mouth; which operation may for greater certainty be tried on each side. If no more teeth are felt in that line, it may be certainly concluded, that those first discovered are what have been distinguished by the name of fangs, and consequently that the serpent is a venomous one. If, on the contrary, the teeth first discovered are found not to stand alone, but to be only a part of a complete row, it may as certainly be concluded that the serpent is not venomous.

In the upper jaw, both of venomous serpents and others, besides the teeth already spoken of, there are 2 interior rows; consequently the distinction endeavoured to be established might be expressed in other words, by saying, that all venomous serpents have only 2 rows of teeth in the upper jaw, and all others have 4. It may be better however to leave the interior rows out of the question, as, in many species, the teeth of which they are composed are so small, as to make it very difficult to discover them. Indeed, in 2 species of anguis, Dr. G. can hardly be sure that he has discovered them; but as, in every other species he has never failed to do so, with very little risk of error he may assert, that all serpents whatever are furnished with them; and that those only which are not venomous have the exterior rows.

What has been said sufficiently shows that Linnæus's ideas, respecting venomous serpents, were such as did not permit him to separate them from the others; if the method above proposed shall be found to render the distinction of them sufficiently clear and easy, it naturally follows that they should be made generically distinct. Some other reforms might also be made in Linnæus's class of amphibia, the consideration of which Dr. G. does not mean at present to enter further into. But, before concluding, he thinks it necessary to notice an inaccuracy of Linnæus of a different kind from those above pointed out. In the Preface to the Museum Regis, and in the Introduction to the class amphibia, in the Systema Naturæ, Linnæus says, that the proportion of venomous ser-

pents to others, is 1 in 10; yet in the Systema Naturæ, in which the sum total of species is 131, he has marked 23 as venomous, which is somewhat more than 1 in 6. However, the last mentioned proportion seems to be not far from the truth; as out of 154 species of serpents Dr. G. examined, he finds 26 to be venomous. It has already been mentioned, that the coluber stolatus and the mycterizans, though marked by Linnæus as venomous serpents, certainly are not so; and Dr. G. suspects the same may be said of the leberis, and dipsas. He has also observed, that the boa contortrix, coluber cerastes, and laticaudatus, none of which are marked in the Systema Naturæ, are all of them venomous; to these last may be added the coluber fulvus.

IV. On the Dryness of the Year 1788. *By the Rev. B. Hutchinson.* p. 37.

As the defect of rain has been very considerable in 1788; and in consequence a great want of water on the close of the year universally felt; perhaps the quantity fallen here, at Kimbolton, compared with that of the 7 preceding years, may not be unacceptable to the Royal Society.

Rain in	Inches.
1781	21.6
1782	32.3
1783	23.6
1784	28.0
1785	21.0
1786	24.7
1787	23.8
1788	14.5

By estimation it therefore appears, that the average quantity of rain of the 7 preceding years is 25 inches, and the rain which fell last year is only 14.5, that is, not much above half that quantity, if we deduct 1.3 now lying in snow, which fell in December, and not in solution. On the supposition, which he believes is not far from truth, that the whole island has had the same defect; a greater failure of the produce of the earth might have been expected than what the country has experienced; for, except in hay, and a little failure in turneps, the crops have in general been as plentiful as in most of the former years, and in fruits of the orchard much more so. It has always been said of England, that drought never occasions want; this year verifies the assertion.

Having premised that there were no extremes of cold and heat throughout the year; the thermometer in a northern exposure never falling below the freezing point during the day-time, except on the 14th and 15th of January, the 6th, 7th, 8th, 10th, 11th, 12th, 13th, and 17th of March, and on none of those days at noon, so that there never were 24 hours together successive frost; therefore vegetation was never entirely at a stand. In summer it did not rise to 80°, except on some few days. Now, the rain that fell on February was towards the end of the month; which, together with that which fell in March, brought up the spring corn, gave an early first crop of hay to the large towns, and covered the meadows and pastures in the country; that they were not so entirely dried up through the defect of April, as to prevent the rain, which fell plentifully on the 29th of May, succeeded by more in June, giving a 2d crop to the former situa-

tions, and a first, though late one, to the latter: and as fructification chiefly depends on rain falling at the latter end of the season of flowering, this rain set the blossoms of wheat, and of the useful fruit-trees; as the great rains in August swelled the kernel, filled, as they term it, the bushel, and gave an opportunity for a 2d crop of turneps that proved more vigorous than the first.

V. On the Method of Determining, from the Real Probabilities of Life, the Value of a Contingent Reversion in which Three Lives are involved in the Survivorship. By Mr. Wm. Morgan. p. 40.

In a paper which Mr. M. lately communicated to the R. S. respecting the method of determining the values of reversions depending on survivorships between 2 persons from the real probabilities of life, he observed, that the investigation of those cases in which 3 lives were involved in the survivorship, though attended with much more difficulty, might however be effected in a similar manner. The further pursuit of this subject convinced him that, as it is never safe, so it can never be necessary to have recourse to the expectations of life in any case; and that the solution even of those problems which include 3 lives, is far from being so formidable as at first sight it appears to be. Mr. M. is sensible of the impropriety of entering minutely in this place into the vast variety of propositions which refer to the different orders of survivorship among 3 lives; but as the following problem seemed to be of considerable importance on account of its being applied to the solution of many other problems, the demonstration of it might not be thought an improper addition to his former paper. The problem is this: Supposing the ages of A, B, and C to be given; to determine, from any table of observations, the value of the sum S payable on the contingency of C's surviving B, provided the life of A shall be then extinct. Of this prob. Mr. M. gives a long and elaborate algebraic investigation. But, for all useful purposes, it may suffice to refer to Mr. Morgan's separate works, as well as to those of Dr. Price, whence ample satisfaction may be obtained.

VI. Result of Calculations of the Observations made at various Places of the Eclipse of the Sun, which happened June 3, 1788. By the Rev. Joseph Piazzi, C. R., Prof. of Astronomy at Palermo.* p. 55.

The methods of computing the longitudes of places, from the observations of solar eclipses, are well known. This M. Piazzi has here undertaken to do for several different places, from which he has collected the observations made on the above mentioned eclipse.

The observations of Loampit-Hill, Greenwich, and Oxford, as they serve for the basis of all his calculations, Mr. P. has calculated them 2 different ways, viz.

* The celebrated discoverer of one of the lately found planets.

by the method of parallactic angles, and by the method of the nonagesimal, and the results agreed together within a few 10ths of a second. By these 2 different methods he also calculated the observations of Vienna, Berlin, and Viviers, in order to show that the different latitudes of the moon, given by the various observations, were not owing to any error in his calculations. For these places, in which both the beginning and the end of the eclipse have been observed, he deduced the time of conjunction from the 2 phases conjointly, which have also given the duration of the eclipse, which cannot be obtained from a single observation.

The error of the tables which results from the observation at Greenwich is $+ 26''$ in longitude, and $+ 11''.5$ in latitude, at $20^h\ 58^m\ 47^s.3$ of apparent time, taking for the longitude of the sun $2^s\ 14°\ 16'\ 54''.7$, as deduced from the Nautical Almanac, and that of the moon at the same time to be greater than the sun by $26''$, as deduced from the same Almanac. He supposed also the horary motion of the moon in the ecliptic, by taking it a half hour before and after the conjunction, to be $36'\ 52'' + 0''.6$ for the hour following the conjunction, and $- 0''.6$ for the hour preceding the conjunction; the moon's horary motion in latitude is $3'\ 24''.3$; the horizontal parallax of the moon minus that of the sun at Greenwich, to be $60'\ 14''.4$ for the commencement of the eclipse, and $60'\ 16''.4$ for the end; the sun's diameter $31'\ 34''.6$, less by $3''$ than that given in the Almanac, according to the correction which Dr. Maskelyne found necessary to be made; the moon's diameter is stated as in the Almanac. In the opinion of M. de la Lande, some correction ought to be made to the parallax and to the diameter of the moon, as well as to the diameter of the sun; but on the one hand this would not make any alteration in the difference of the meridians which Mr. P. has found; and on the other he thought proper to make use of those elements the Nautical Almanac furnished, that being a work the most perfect of the kind that ever appeared, and to which all astronomers and navigators ought to pay the greatest attention.

In fine, he compared the moon's longitude in conjunction deduced from the eclipse with the new tables of the moon corrected by Mr. Mason, and found the longitude by those tables to be $2^s\ 14°\ 17'\ 6''.4$, and the latitude to be $15'\ 1''.3$. The error then of the new tables is $+ 11''.7$ in longitude, and $+ 13''.1$ in latitude; but M. de la Lande having lately sent to him from Paris the place of the sun, calculated with the new solar tables (a most useful improvement which M. de Lambre has, with much ingenuity, deduced from observations) he finds the error in longitude to be $+ 27''.4$, the sun's place being $2^s\ 14°\ 16'\ 39''.0$ at $20^h\ 58^m\ 47^s.3$.

The following table contains the observations of the eclipse, and the results deduced from them. The first vertical column shows the name and place of the

observers; the next 2 vertical columns contain all those observations which have been made, in apparent time; the other columns show the results, viz. the 4th column contains the true conjunction in apparent time; the 5th column contains the latitude of the moon, which, as it depends on the manner of observing the 2 phases, is subject to some variety; the 6th or last column contains the difference between the various meridians and that of Greenwich.

Table of the Observations made at Various Places on the Eclipse of the Sun, which happened June 3, 1788, and of results deduced from the same. Longitude of the Moon in Conjunction being 2ˢ 14° 16′ 54″.7.

Observers.	Beginning.	End.	Conjunction	Latitude in conjunction.	Difference of meridians.
Greenwich, Dr. Maskelyne	19ʰ 24ᵐ 46ˢ.5	21ʰ 1ᵐ 24ˢ.0	20ʰ 58ᵐ 47ˢ.3	14′ 48″.2	0ʰ 0ᵐ 0ˢ
Loampit-Hill, Mr. Aubert........	19 24 41 .9	21 1 20 .3	20 58 44 .1	14 48 .2	0 0 3 .2W.
Oxford, Dr. Hornsby............	19 20 36 .1	20 54 40 .0	20 53 46 .2	14 48 .7	0 5 1 .1W.
Dublin, Dr. Ussher.............	19 5 46 .5	20 27 42 .1	20 33 33 .9	14 48 .3	0 25 13 .4W.
Mittau, M. Beitler.............	21 20 15 .0	23 8 52 .0	22 33 41 .5	14 48 .7	1 34 54 .2E.
Berlin, M. Bode................	20 23 9 .0	22 14 32 .0	21 52 20 .3	14 44 .2	0 53 33 E.
Vienna, M. Triesneker	20 25 49 .0	22 32 40 .0	22 4 18 .8	14 39 .0	1 5 31 .5E.
Viviers, M. Flaugerguas..........	19 26 38 .0	21 25 41 .0	21 17 29 .0	14 33 .0	0 18 41 .7E.
Perinaldo, M. Maraldi............	19 37 50 .0	*	21 29 40	*	0 30 53 .0E.
Rouen, M. Du Lange............	*	21 7 15 .0	21 3 9 .6	*	0 4 22 .3E.
Milan, Mess. De Cesaris and Reggio	19 48 23 .0	21 51 14 .0	21 35 24 .7	14 32 .0	0 36 37 .4E.
Bologna, M. Matteucci	19 55 10 .5	22 3 45 .5	21 44 15 .3	14 31 .0	0 45 28 E.
Padua, M. Chiminello............	19 59 20 .0	22 6 58 .0	21 46 21 .3	14 39 .0	0 47 34 E.
Warsaw, M. Bystrzyski	20 56 45	22 57 33	22 22 59 .3	14 44	1 24 12
Prague, M. Strnadt.............	20 21 29	22 21 15	21 56 30	14 45	0 57 42 .7
Marseilles, M. Bernard	19 26 42	21 29 23 .5	21 20 17 .5	14 40	0 21 30 .2
Cresmunster, M. Fixlmillner......	20 15 20	22 19 50 .7	21 54 59	14 23	0 56 11 .7
Bagdad, M. De Beauchamp	22 30 51	23 26 19	23 56 11	*	2 57 23 .7

VII. Of a Bituminous Lake or Plain in the Island of Trinidad. By Mr. Alex. Anderson. p. 65.

A most remarkable production of nature in the island of Trinidad, is a bituminous lake, or rather plain, known by the name of Tar Lake; by the French called La Bray, from the resemblance to, and answering the intention of, ship pitch. It lies in the leeward side of the island, about half way from the Bocas to the south end, where the Mangrove swamps are interrupted by the sand-banks and hills; and on a point of land which extends into the sea about 2 miles, exactly opposite to the high mountains of Paria, on the north side of the Gulf. This cape, or head-land, is about 50 feet above the level of the sea, and is the greatest elevation of land on this side of the island. From the sea it appears a mass of black vitrified rocks; but on a close examination it is found a composition of bituminous scoriæ, vitrified sand, and earth, cemented together; in some parts beds of cinders only are found. In approaching this Cape, there is

a strong sulphureous smell, sometimes disagreeable. This smell is prevalent in many parts of the ground to the distance of 8 or 10 miles from it.

This point of land is about 2 miles broad, and on the east and west sides, from the distance of about half a mile from the sea, falls with a gentle declivity to it, and is joined to the main land on the south by the continuation of the Mangrove swamps; so that the bituminous plain is on the highest part of it, and only separated from the sea by a margin of wood which surrounds it, and prevents a distant prospect of it. Its situation is similar to a Savannah, and, like them, it is not seen till treading on its verge. Its colour, and even surface, present at first the aspect of a lake of water; but probably it got the appellation of lake when seen in the hot and dry weather, at which time its surface to the depth of an inch is liquid, and then from its cohesive quality it cannot be walked on. It is of a circular form, and about 3 miles in circumference. At the first approach it appeared a plane, as smooth as glass, excepting some small clumps of shrubs and dwarf-trees that had taken possession of some spots of it; but when Mr. A. had proceeded some yards on it, he found it divided into areolæ of different sizes and shapes: the chasms or divisions anastomosed through every part of it; the surface of the areolæ perfectly horizontal and smooth; the margins undulated, each undulation enlarged to the bottom till they join the opposite. On the surface the margin or first undulation is distant from the opposite from 4 to 6 feet, and the same depth before they coalesce; but where the angles of the areolæ oppose, the chasms or ramifications are wider and deeper. When he was at it, all these chasms were full of water, the whole forming one true horizontal plane, which rendered the investigation of it difficult and tedious, being necessitated to plunge into the water a great depth in passing from one areola to another. The truest idea that can be formed of its surface will be from the areolæ and their ramifications on the back of a turtle. Its more common consistence and appearance is that of pit-coal, the colour rather greyer. It breaks into small fragments, of a cellular appearance and glossy, with a number of minute and shining particles interspersed through its substance; it is very friable, and, when liquid, is of a jet black colour. Some parts of the surface are covered with a thin and brittle scoria, a little elevated. As to its depth he could form no idea of it; for in no part could he find a substratum of any other substance; in some parts he found calcined earth mixed with it.

Though he smelt sulphur very strong on passing over many parts of it, he could discover no appearance of it, or any rent or crack through which the steams might issue; probably it was from some parts of the adjacent woods: for though sulphur is the basis of this bituminous matter, yet the smells are very different, and easily distinguished, for its smell comes the nearest to that of pitch of any thing. He could make no impression on its surface without an

axe: at the depth of a foot he found it a little softer, with an oily appearance, in small cells. A little of it held to a burning candle makes a hissing or cracking noise like nitre, emitting small sparks with a vivid flame, which extinguishes the moment the candle is removed. A piece put in the fire will boil up a long time without suffering much diminution: after a long time's severe heat, the surface will burn and form a thin scoria, under which the rest remains liquid. Heat seems not to render it fluid, or occupy a larger space than when cold; whence he imagines there is but little alteration on it during the dry months, as the solar rays cannot exert their force above an inch below the surface. He was told by one Frenchman, that in the dry season the whole was a uniform smooth mass; and by another, that the ravins contained water fit for use during the year; but can believe neither: for if, according to the first assertion, it was an homogeneous mass, something more than an external cause must affect it, to give it the present appearances: nor without some hidden cause can the 2d be granted. Though the bottoms of these ramified channels admitted not of absorption, yet from their open exposure, and the black surface of the circumjacent parts, evaporation must go on amazing quick, and a short time of dry weather must soon empty them; and from the situation and structure of the place there is no possibility of supply but from the clouds. To show that the progress of evaporation is amazingly quick here, at the time he visited it there were, on an average, $\frac{1}{3}$ of the time incessant torrents of rain; but from the afternoon being dry, with a gentle breeze, as is generally the case during the rainy season in this island, there evidently was an equilibrium between the rain and the evaporation; for in the course of 3 days he saw it twice, and perceived no alteration on the height of the water, nor any outlet for it but by evaporation.

Mr. A. takes this bituminous substance to be the bitumen asphaltum Linnei. A gentle heat renders it ductile; hence, mixed with a little grease or common pitch, it is much used for the bottoms of ships, and for which intention it is collected by many, and he conceives it a preservative against the Borer, so destructive to ships in this part of the world. Besides this place, where it it found in this solid state, it is found liquid in many parts of the woods; and at the distance of 20 miles from this about 2 inches thick, round holes of 3 or 4 inches diameter, and often at cracks or rents. This is constantly liquid, and smells stronger of tar than when indurated, and adheres strongly to any thing it touches; grease is the only thing that will divest the hands of it.

The soil in general, for some distance round La Bray, is cinders and burnt earths; and where not so, it is a strong argillaceous soil; the whole exceedingly fertile, which is always the case where there are any sulphureous particles in it. Every part of the country, to the distance of 30 miles round, has every appear-

ance of being formed by convulsions of nature from subterraneous fires. In several parts of the woods are hot springs; some he tried, with a well graduated thermometer of Fahrenheit, were 20° and 22° hotter than the atmosphere at the time of trial. From its position to them, this part of the island has certainly experienced the effects of the volcanic eruptions, which have heaped up those prodigious masses of mountains that terminate the province of Paria on the north; and no doubt there has been, and still probably is, a communication between them. One of these mountains opposite to La Bray in Trinidad, about 30 miles distant, has every appearance of a volcanic mountain: however, the volcanic efforts have been very weak here, as no trace of them extend above 2 miles from the sea in this part of the island, and the greater part of it has had its origin from a very different cause to that of volcanos; but they have certainly laid the foundation of it, as is evident from the high ridge of mountains which surrounds its windward side to protect it from the depredations of the ocean, and is its only barrier against that over-powering element, and may properly be called the skeleton of the island.

From every examination Mr. A. has made, he finds the whole island formed of an argillaceous earth, either in its primitive state, or under its different metamorphoses. The bases of the mountains are composed of schistus argillaceus and talcum lithomargo; but the plains or low-lands remaining nearly in the same moist state as at its formation, the component particles have not experienced the vicissitudes of nature so much as the more elevated parts, consequently retain more of their primitive forms and properties. As argillaceous earth is formed from the sediment of the ocean, from the situation of Trinidad to the continent, its formation is easily accounted for, granting first the formation of the ridge of mountains that bound its windward side, and the high mountains on the continent that nearly join it: for the great influx of currents into the Gulf of Paria, from the coasts of Brazil and Andalusia, must bring a vast quantity of light earthy particles from the mouths of the numerous large rivers which traverse these parts of the continent; but the currents being repelled by these ridges of mountains, eddies and smooth water will be produced where they meet and oppose, and therefore the earthy particles would subside, and form banks of mud, and by fresh accumulations added would soon form dry land; and from these causes it is evident such a tract of country as Trinidad must be formed. But these causes still exist, and the effect from them is evident; for the island is daily increasing on the leeward side, as may be seen from the mud-beds that extend a great way into the Gulf, and there constantly increase. But from the great influx from the ocean at the south end of the island, and its egress to the Atlantic again, through the Bocas, a channel must ever exist between the continent and Trinidad.

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VIII. *Of a particular Change of Structure in the Human Ovarium. By Matthew Baillie, M. D.* p. 71.

The ovaria in women are subject to a great variety of changes from their natural structure. Many of these are exactly similar to what take place in other parts of the body; but there is one which seems peculiar to them, the nature of which has probably not been hitherto very well ascertained. The change of structure here alluded to, is a conversion of the natural substance of an ovarium into a fatty mass, intermixed with hair and teeth. This sort of change is rare, though it occurs sufficiently often to have been seen by most persons who are very conversant in the examination of dead bodies. There are many cases of it related in the different books of dissections, but most commonly without any remarks on the mode of formation;* or they have been considered as very imperfect attempts at the growth of a fœtus in the ovarium, in consequence of connection between a male and a female. This conjecture rests no doubt on strong circumstances of probability, and yet there are many powerful reasons which seem to oppose its being well founded. Generation is a process always depending on the action of a certain cause, viz. the usual connection between a male and a female; and when effects similar to those in generation are perceived, it becomes very natural to conclude that this cause has been employed. The bias to such an opinion will become the stronger, from reflecting on the passions that are known to influence so powerfully mankind, by which the agency of this cause is frequently excited. When a change therefore was observed in an ovarium, by which it was converted into a fatty mass with hair and teeth, this should seem to correspond so much with a change taking place in consequence of generation, that the mind would scarcely entertain a doubt of its arising from the same cause, and would readily infer, that it had been preceded by a connection between the sexes. This doubt would still be the less, from the circumstance of a complete fœtus being sometimes formed in the ovarium, where the usual means of generation had been employed. The following case however exhibits many reasons why we should be led to believe, that the ovaria in women have some power within themselves of taking on a process which is imitative of generation, without any previous connection with a male; and it is with this view that it is here related.

In a female child, about 12 or 13 years old, which was brought to Windmill-street for dissection, Dr. B. found the right ovarium converted into a substance, doughy to the touch, and about the size of a large hen's egg. On

* It has been the opinion of some, that hair, teeth, nails, feathers, &c. are animal vegetables or plants; and, agreeably to this opinion, Dr. Tyson considers the growth of hair and teeth in the ovarium as a lusus naturæ, where nature endeavours to produce something, and being disappointed in forming an animal, produces a vegetable. Vide Hooke's Lectures and Collection, N° 2, p. 11 and 15.—Orig.

cutting into the substance, he found an apparently fatty mass, intermixed with hair and an excrescence of bones. This startled him very much, as he had always been led to believe that such appearances were a sort of imperfect conception. The circumstances together being very singular, he was led to pay considerable attention to the change in the ovarium. The fatty mass was of a yellowish white colour, in some places more yellow than in others, was very unctuous to the feeling, and consisted of shortened or separated particles, not having the same coalescence which the fat has generally in the body. It became very soft when exposed to the heat of a fire, and sunk into a portion of paper, on which it was spread, so as to make it more transparent. When the paper to which it was applied was exposed to the flame of a candle, it burnt with considerable crackling. The hair with which the fatty substance was mixed grew out of the inner surface of the capsule containing it, in some places in solitary hairs, but chiefly in small fasciculi, at scattered irregular distances. Besides these, there were loose hairs involved in the fatty mass. The hairs were, some of them of considerable length, even to 3 inches, were fine, and of a light brown colour. They resembled much more the hairs of the head, than what are commonly found on the pubis, and corresponded very much in colour to the hair of the girl's head.

There arose also from the inner surface of the capsule some vestiges of human teeth. One appeared to be a canine tooth, another to be a small grinder, 2 others to be incisors, and there was also a very imperfect attempt at the formation of another tooth. These were not fully formed, the fangs being wanting; but in 2 of them the bodies were as complete as they are ever found in the common circumstances. They were each of them inclosed in a proper capsule, which arose from the inner surface of the ovarium, and consisted of a white thick opaque membrane. Attached to the capsules of 3 of the teeth, there was a white spongy substance. The membrane of the ovarium itself was of some considerable thickness, but unequal in the different parts, was very smooth in its inner surface, and more irregular externally. The uterus was smaller than it is commonly at birth, was perfectly healthy in its structure, and on opening into its cavity it exhibited the ordinary appearances of a child's uterus at that period. The left ovarium was very small, corresponding to the state of the uterus. It appears clearly from this, that the uterus had not yet received the increase of bulk, which is usual at the age of puberty. The hymen was entire, such as is commonly found in a child of the same age; and there was just beginning a lanugo on the labia, not more than what is often found on the upper lip of a boy of 15 years old. Such are the circumstances attending this singular case, and they present to the mind various grounds of consideration.*

* See also a remarkable instance of an ovarium containing teeth, hair, and bones, related by Mr. Cleghorn, in the first vol. of the Transactions of the Royal Irish Academy.

The formation of hair and teeth is a species of generation, for in fact it makes a part of it, and strikes the mind as being very different from any irregular substance which is formed by disease. This formation too takes place in a part of the body which is subservient to generation, and where a complete fœtus is sometimes formed. The whole of this looks very much as if the production of hair and teeth in the ovarium was a sort of imperfect impregnation. But when we take another view of it, there are reasons at least equally strong for believing that such productions may arise from an action in the ovarium itself, without any stimulus from the application of the male semen.

In the case before us, the uterus was as small as at birth, indeed more so, and the left ovarium, which was perfectly healthy, corresponded to the state of the uterus. It had not been at all stimulated, nor did appear capable of being stimulated by the application of the male semen. This seems to be a strong circumstance; for in a case where there was an ovum formed in one of the Fallopian tubes, the uterus was enlarged to more than twice its unimpregnated size; and, on opening into its cavity, the decidua was observed to be formed as completely as in the impregnated uterus. This preparation is still preserved in the collection of Windmill-street. Nothing can be a stronger proof, that when an impregnation takes place out of the cavity of the uterus, the uterus still takes a share in the action, and undergoes some of the changes of impregnation. In another preparation, which is preserved in the same collection, where there was a fœtus formed in the ovarium, the uterus was increased to more than twice its ordinary size, was very thick and spongy, and had its blood-vessels enlarged as in an impregnated uterus. This becomes another very strong proof of the action of the uterus in the formation of an extra-uterine fœtus. In the case before us however, the uterus had undergone no change, and does not seem to have arrived at that period when it could be capable of undergoing such a change.

Besides, we are not to consider the formation of teeth in the ovarium to be a quicker process than it is commonly in the head of a fœtus; but in the present case the teeth having advanced fully as far as they are at some months after birth, this process must have begun at least more than a twelvemonth before the death of the child. If then we consider it as an impregnation, since the appearances of the child do not warrant us to believe her to have been more than 12 or 13 years old, this brings the date of the impregnation to an earlier period than can well be believed. From all these circumstances we might be led to suppose, that the formation of the hair and teeth was not in consequence of any connection with a male, but arose from some action of the ovarium itself, in which the uterus did not participate. The existence of the hymen, especially in so young a girl, becomes a collateral confirmation of the same opinion, though much is not to be rested on it, when taken singly.

It will perhaps have some influence in removing the prejudices against this opinion, to make the following remarks. Hair is occasionally formed in parts of the human body, which are absolutely unconnected with generation. Encysted tumours are sometimes found containing hair. Mr. Hunter has a preparation of this sort in his collection, which he cut out from under the skin of the eyebrow. This tumour was perfectly complete, and unconnected with the skin, except by the common intervention of cellular membrane, so as to have no communication whatever with the hair of the eyebrow. In this instance there was certainly a species of generation taking place in the encysted tumour itself, forming hairs as completely and fully as in the common progress of the formation of a child. Such encysted tumours have been found in other parts of the human body, and still more frequently in quadrupeds. Mr. Hunter has in his collection many specimens of encysted tumours from cows and sheep containing hair and wool. These were perfectly complete, so as to have possessed a power of production within themselves, and were many of them found deeply seated at a considerable distance from the skin, which is the common parent of hair. In these tumours there is often the appearance of layers of cuticle, which is probably a preparatory step to the formation of hair. All this shows most clearly, that hair may be formed without any species of generation as it is commonly understood. But hair is in itself as distinct a consequence of generation as teeth, and as much a peculiar substance. If then the one be formed, there appears to be no reason why the other should not also be formed. The action producing the one is not better understood than that producing the other; nor does it appear to be really in itself less connected with that species of generation arising from the approach of a male, so that teeth may probably be formed by a peculiar action taking place in the ovarium itself, as well as the hair.

It will tend to add further weight to this opinion, to consider that many of the adult teeth are formed in a child after birth; and therefore their formation depends on an action taking place in the jaws at a particular period, and not on original growth. The same circumstance strikes more strongly in the occasional formation of teeth at an advanced time of life. Both of these processes take place after the animal has been formed, in consequence of a certain action being excited in a particular part of the body, and therefore there is less difficulty in believing that the same sort of process may go on in another part of the body not commonly employed in it. It seems reasonable also to suppose, that the ovaria should have a greater aptitude of taking on a process somewhat similar to generation than the other indifferent parts of the body, as they constitute a part of the organs which are so materially concerned in the real process itself*. These cir-

* As the formation of teeth and hair involved in a fatty mass may be said to be peculiar to the ovaria, and as there are strong reasons for believing that this formation may take place without an in-

cumstances, when taken collectively, would seem to render it very probable, that the formation of hair and teeth in the ovarium does not necessarily depend on a connection between a male and a female, as has been the common opinion, but arises from some action within the ovarium itself, which is imitative of generation.

IX. On the Vegetable and Mineral Productions of Boutan and Thibet. By Mr. Robert Saunders, Surgeon at Boglepoor in Bengal. p. 79.

Road to Buxaduar, May 11 and 12, 1783. The tract of country from Bahar to the foot of the hills contains but few plants that are not common to Bengal. Pine-apples, mango-tree, jack and saul timber, are frequently to be met with in the forests and jungles. Many orange-trees towards the foot of the hills, of a very good sort, and bearing much fruit. Saw a few lime-trees, and found 3 different species of the sensitive plant. One species is used medicinally by the natives of Bengal in fevers; it is a powerful astringent and bitter; another is the species from which Terra Japonica is made, a medicine the history of which we are but lately made acquainted with. The 3d species is well known as the sensitive plant, and common in Bengal.

The country, from Bahar to the foot of the mountains, to which we approach without any ascent, is rendered one of the most unhealthy parts of India, from a variety of causes. The whole, a perfect flat, is at all times wet and swampy, with a luxuriant growth of reeds, long grass, and underwood, in the midst of stagnated water, numerous frogs and insects. The exhalations from such a surface of vegetable matter and swamps, increased by an additional degree of heat from the reflection of the hills, affect the air to a considerable extent, and render it highly injurious to strangers and European constitutions. The thermometer at the foot of the hill, mid-day 86°, fell to 78° at 2 o'clock, the time we reached Buxaduar, and that hour of the day when it is generally highest.

Buxaduar, May 12 to 21. Many of the plants peculiar to Bengal require nursing at Buxaduar. There is one very good banian tree. In the jungles, met with the ginger, and a very good sort of yam; saw some pomegranate-trees in good preservation; shallots in great perfection; a species of the lychnis, arum, and asclepias, natives of more northern situations, and of little use; a bad sort of rasberry, and a species of the gloriosa. The plantains in use below do not thrive here. In the jungles they have a plantain-tree producing a very broad leaf, with which they cover their huts; but the fruit is not eaten. From the 15th to the 22d, the rains were almost incessant at Buxaduar. Our people became unhealthy,

tercourse between the sexes, it becomes difficult to account for this peculiarity in them, unless by supposing that they have a greater aptitude of running into such a process, than the other parts of the body.—Orig.

and were attacked with fevers, which, if neglected in the beginning, prove obstinate quartans. Buxaduar lies high, but is overtopped by the surrounding mountains, covered with forests of trees and underwood. In all climates, where the influence of the sun is great, this is a never-failing cause of bad air. The exhalation that takes place from so great a surface in the day-time falls after sunset in the form of dew, rendering the air raw, damp, and chilly, even in the most sultry climates. The thermometer at Buxaduar was never, at 2 o'clock in the afternoon, above 82°, nor below 73°.

Road to Murishong, May 22 and 23. In ascending the hill from Buxaduar there is to be seen much of an imperfect quartz, of various forms and colour, having in some places the appearance of marble; but from chemical experiments, it was found to possess very different properties. This sort of quartz, when of a pure white, and free from any metallic colouring matter, is used as an ingredient in porcelain. It is known to mineralists in that state by the name of quartz gritstone. The rock which forms the basis of these mountains dips in almost every direction, and is covered with a rich and fertile soil, but in no place level enough to be cultivated. Many European plants are met with on the road to Murishong; many different sorts of mosses, fern, wild thyme, peaches, willow, chickweed, and grasses common to the more southern parts of Europe, nettles, thistles, dock, strawberry, rasberry, and many destructive creepers, some common in Europe.

Murishong is the first pleasant and healthy spot to be met with on this side of Boutan. It lies high, and much of the ground about it is cleared and cultivated; the soil, rich and fertile, produces good crops. The only plant now under culture is a species of the polygonum of Linneus, producing a triangular seed, nearly the size of barley, and the common food of the inhabitants. It was now the beginning of their harvest; and the ground yields them, as in other parts of Boutan, a 2d crop of rice. Here are to be found in the jungles 2 species of the laurus of Linneus; one known by the name of the bastard cinnamon. The bark of the root of this plant, when dried, has very much the taste and flavour of cinnamon; it is used medicinally by the natives. The chenopodium, producing the semen santonicum, or worm-seed, a medicine formerly in great character, and used in those diseases from which it is named, is common here. Found in the neighbourhood of this place all the European plants we had met with on the road. The ascent from Buxaduar to Murishong is on the whole great, with a sensible change in the state of the air.

Road to Chooka, May 25. On the road to Chooka find all the Murishong plants, cinnamon-tree, willow, and 1 or 2 firs; strawberries every where, and very good, and a few bilberry plants. Much sparry flint, and a sort of granite with which the road is paved. There is a great deal of talc in the stones and soil, but in too small pieces to be useful. Frequent beds of clay and pure sand.

Find 2 mineral wells, slightly impregnated with iron, with much appearance of that metal in this part of the country; and they are not unacquainted with the method of extracting it from the stones, but still despise its use in building. Towards Chooka there are many well cultivated fields of wheat and barley.

Road to Punukha, May 26. From Chooka the country opens, and presents to view many well cultivated fields and distant villages; a rapid change in climate, the vegetable productions, and general appearance of the country. Towards Punukha, pines and firs are the only trees to be met with; but they do not yet seem in their proper climate, being dwarfish and ill-shaped: peaches, rasberries, and strawberries, thriving every where; scarcely a plant to be seen that is not of European growth. In addition to the many already mentioned, saw 2 species of the cratægus, one not yet described. Saw 2 ash-trees in a very thriving state, the star-thistle, and many other weeds, in general natives of the Alps and Switzerland.

Much of the rock is pure lime-stone; a valuable acquisition if they did not either despise its use, or were unacquainted with its properties. It was most advantageously situated for being worked, and the purest perhaps to be met with. There is likewise abundance of fire-wood in this part of the country. In building they would derive great benefit from the use of it. Their houses are lofty, the timbers substantial, and nothing wanting to make them durable, but their being acquainted with the use of lime. As a manure it might probably be used to great advantage. Many fields of barley in this part of the country; now the beginning of their harvest. The thermometer here fell, at 4 o'clock in the afternoon, to 60°: cold and chilly.

Road to Chepta, May 27. On the road to Chepta, the rock in general dips to the northward and eastward, in about an angle of 60 degrees. Much of limestone, and some veins of quartz, and loose pieces of sparry flint striking fire with steel. Several springs, and one slightly impregnated with iron. In addition to the plants of yesterday, found the coriandrum testiculatum, inula montana, and rhododendrum magnum. At Chepta met with a few turneps, one maple-tree, worm-wood, goose-grass (galium aparine), and many other European weeds; the first walnut-tree we had yet seen. Chepta lies high, and not above 6 miles from the mountain of Lomyla, now covered with snow. The wind from that quarter, S.E. made it cold and chilly, and sunk the thermometer at mid-day to 57°. Here are some fields of wheat and barley not yet ripe.

Road to Pagha, May 29. Soon after leaving Chepta find a mineral well, which, on a chemical examination, gave marks of a strong impregnation from iron. Traced it to its source, where the thermometer, on being immersed, fell from 68° to 56°. A little before we reach Pagha, met with some lime-stone, and a bed of chalk, which, near the surface, contained a great proportion of sand, but

some feet under was much purer. The forests of firs of an inferior growth, several ash-trees, dog-rose, and bramble.

Road to Tassesudon, May 30 and 31, June 1. The road from here to Tassesudon presents us with little that we have not met with; fewer strawberries, and no rasberries; some very good orchards of peaches, apricots, apples, and pears. The fruit formed, and will be ripe in August and September. Met with two sorts of cranberry, one very good. Saw the fragaria sterilis, and a few poppies. At Wanakha found a few turnips, shallots, cucumbers, and gourds. Near to Tassesudon the road is lined with many different species of the rose, and a few jessamine plants. The soil is light, and the hills in many places barren, rocky, and with very little verdure. The rock in general laminated and rotten, with many small particles of talc in every part of the country incorporated with the stones and soil. Some lime-stone, and appearance of good chalk. Several good and pure springs of water. The hills are chiefly wood, with firs and aspen. Have not yet been able to find an oak-tree, and the ash very seldom. The elder, holly, bramble, and dog-rose, are common. Found the birch-tree, cypress, yew, and delphinium. Many different species of the vaccinium, among them the bilberry and the cranberry. Towards the top of the adjacent mountains met with two plants of the arbutus uva ursi, which is a native of the Alps, the most mountainous parts of Scotland, and Canada. Have seen a species of the rhubarb plant (rheum undulatum) brought from a distance, and only to be met with near the summits of hills covered with snow, and where the soil is rocky. The true rhubarb (rheum palmatum) is also the native of a cold climate; and though China supplies us with much of this drug, it is known to be the growth of its more northern provinces, Tartary, and part of the Russian dominions. The great difficulty is in drying the root. People versant in that business say, that 100lb. of the fresh root should not weigh above $6\frac{1}{4}$lb. if properly dried, and it certainly has been reduced to that. Have seen 80lb. of fresh root produced from 1 plant; but, after drying it with much care and attention, the weight of the dried root could not be made less than 12lb. It was suspended in an oven, with an equal and moderate degree of heat. Little more than the same quantity of this powder produced a similar effect with the best foreign rhubarb.

The other plants common here are the service-tree, blessed thistle, mock orange, spiræa filipendula, arum, echites, punica, ferula communis, erica, and viola. Of the rose-bush I have met with the 5 following species; rosa alpina, centifolia, canina, indica, spinosissima. The culture of pot-herbs is every where neglected; turnips, a few onions and shallots, were the best we could procure.

Mr. Bogle left potatoes, cabbage, and lettuce-plants, all which we found neglected and dispersed. They had very improperly, from an idea most probably of their being natives of Bengal, planted them in a situation and climate which

approaches very near to that of Bengal at all seasons. Melons, gourds, brinjals, and cucumbers, are occasionally to be met with. The country is fitted for the production of every fruit and vegetable common without the tropics, and in some situations will bring to perfection many of the tropical fruits.

There are 2 plants which I have to regret the not having had as yet an opportunity of seeing; one is the tree from the bark of which their paper is made; and the other is employed by them in poisoning their arrows. This last is said to come from a very remote part of the country. They describe it as growing to the height of 3 or 4 feet, with a hollow stalk. The juice is inspissated, and laid as a paste on their arrows. Fortunately for them, it has not all the bad effects they dread from it. I had an opportunity of seeing several who were wounded with these arrows, and they all did well, though under the greatest apprehension. The cleaning and enlarging some of the wounds was the most that I found necessary to be done. The paste is pungent and acrid, will increase inflammation, and may make a bad or neglected wound mortal; but it certainly does not possess any specific quality as a poison.

The fir, so common in this country, is perhaps the only tree they could convert to a useful and profitable purpose. What I have seen would not, from their situation, be employed as timber. The largest I have yet met with were near Wandepore; they measured from 8 to 10 feet in circumference, were tall and straight. Such near the Burrampooter, or any navigable river, might certainly be transported to an advantageous market. I am convinced that any quantity of tar, pitch, turpentine, and resin, might be made in this country, much to the emolument of the natives. Firs, which from their size and situation are unfit for timber, would answer the purpose equally well. The process for procuring tar and turpentine is simple, and does not require the construction of expensive works. This great object has been so little attended to, that they are supplied from Bengal with what they want of these articles.

The country about Tassesudon contains great variety of soil, and much rock of many different forms, but still an unpromising field for a mineralist. I have not found in Boutan a fossil that had the least appearance of containing any other metal than iron, and a small portion of copper. From information, and the reports of travellers, I believe it is otherwise to the northward. The banks of the Ticushu, admitting of cultivation for several miles above and below Tassesudon, yield them 2 crops in the year. The first of wheat and barley is cut down in June; and the rice, planted immediately after, enjoys the benefit of the rains. This country is not without its hot wells, as well as many numerous springs. One hot well, near Wandepore, is so close to the banks of the river as to be overflowed in the rains, and it was impossible to get to it: the heat of this well is great; but I could not learn that the ground about it was much different from the general

aspect of the country. Another, several days journey from hence, is on the brow of a hill perpetually covered with snow. This hot well is held in great estimation by the people of the country, and resorted to by valetudinarians of every description.

Tassesudon to Paraghon, Sept. 8 and 9. Much good rich soil, with more pasture, where the ground is not cultivated, than we had yet met with. Many fields of turnips in great perfection; a plant they seem better acquainted with the cultivation of than any other. Find on the road many large and well thriving birch, willows, pines, and firs, some walnut-trees, the arbutus uva ursi, abundance of strawberry, elderberry, bilberry, chrysanthemum or greater daisy, and many European grasses. See the datura ferox or thorn apple, a plant common in China and some parts of Thibet, where it is used medicinally. They find it a powerful narcotic, and give the seeds where they wish that effect to be produced. It has been used as a medicine in Europe, and is known to possess these qualities in a high degree. See holly, dog rose, and aspen. The present crop near Paraghon, on the banks of the Pachu, is rice, but not so far advanced as at Tassesudon; the same may be said of their fruits. They say it is colder here at all seasons than at Tassesudon, which is certainly below the level of this place. Towards the summit of the mountain we crossed, found some rock of a curious appearance, forming in front 6 or 7 angular semi-pillars, of a great circumference, and some hundred feet high. This natural curiosity was detached in part from the mountain, and projected over a considerable fall of water, which added much to the beautiful and picturesque appearance of the whole. Numerous springs, some degrees colder than the surrounding atmosphere, gushing from the rock on the most elevated part of the mountain, furnish a very ample and seasonable supply of excellent water to the traveller. The rock, in many places laminated, might be formed into very tolerable slate. Near to Paraghon iron stones are found, and one spring highly impregnated with this mineral.

Road to Dukaigun, Sept. 11. Our road to Dukaigun, nearly due north, is a continued ascent for 8 miles, along the banks of the Pachu, falling over numerous rocks, precipices, and huge stones. Here we begin to experience a very considerable change in the temperature of the atmosphere; the surrounding hills were covered with snow in the morning, which had fallen the preceding night, but disappeared soon after sunrise. The thermometer fell to 54° in the afternoon, and did not rise above 62° at noon. The face of the mountains, in some places bare, with projecting rock of many different forms; quartz, flint, and a bad sort of freestone, common. Many very good springs, slightly impregnated with a selenitic earth. The soil is rich, and near the river in great cultivation. Many horses, the staple article of their trade, are bred in this part of the country. Found walnut-trees, peaches, apples, and pears.

Road to Sanha, Sept. 12. The road still ascending to Sanha, and near the river for 10 miles. The thermometer falling some degrees, we found it cold and chilly. The bed of the river is full of large stones, probably washed down from the mountains by the rapidity of its stream; they are chiefly quartz and granite. Here is excellent pasture for numerous herds of goats.

Road to Chichakumboo. From Sanha the ascent is much greater, and, after keeping for 10 miles along the banks of the Pachu, still a considerable stream, we reach its source, from 3 distinct rivulets, all in view, ramified and supplied by numerous springs, and soon after arrive at the most elevated part of our road. Here we quit the boundary of Boutan, and enter the territory of Thibet, where nature has drawn the line still more strongly, and affords perhaps the most extraordinary contrast that takes place on the face of the earth. From this eminence are to be seen the mountains of Boutan, covered with trees, shrubs, and verdure to their tops, and on the south side of this mountain to within a few feet of the ground on which we tread. On the north side the eye takes in an extensive range of hills and plains, but not a tree, shrub, or scarcely a tuft of grass to be seen. Thus, in the course of less than a mile, we bid adieu to a most fertile soil, covered with perpetual verdure, and enter a country where the soil and climate seem inimical to the production of every vegetable. The change in the temperature of the air is equally obvious and rapid. The thermometer in the forenoon 34°, with frost and snow in the night-time. Our present observations on the cause of this change confirmed us in a former opinion, and incontestably prove, that we are to look for that difference of climate from the situation of the ground as more or less above the general level of the earth. In attending to this cause of heat or cold, we must not allow ourselves to be deceived by a comparison with that which is immediately in view. We ought to take in a greater range of country, and where the road is near the banks of a river, we cannot well err in forming a judgment of the inclination of the ground. Punukha and Wandepore, both to the northward of Tassesudon, are quite in a Bengal climate. The thermometer at the first of these places, in the months of July and January, was within 2° of what it had been at Rungpore for the same periods. They seem in more exposed situations than Tassesudon; and, were we to draw a comparison of their heights from the surrounding ground, I should say they were above its level. The road however proves the reverse. From Punukha to Tassesudon we had a continued and steep ascent for $6\frac{1}{2}$ hours, with a very inconsiderable descent on the Tassesudon side. From the south side of the mountain dividing Boutan from Thibet the springs and rivulets are tumbling down in cascades and torrents, and have been traced by us near to the foot of the hills, where they empty themselves to the eastward of Buxaduar. On the north side they glide smoothly along, and by passing to the northward as far as Tishoo-

lumboo, prove a descent on that side, which the eye could not detect. This part of the country, being the most elevated, is at all times the coldest; and the snowy mountains, from their heights and bearings, notwithstanding the distance, are certainly those seen from Purnea. The soil on the Thibet side of the mountain is sandy, with much gravel and many loose stones. On the road found the aconitum pyreneum, and 2 species of the saxifraga. See a large flock of chowry tailed cattle; their extensive range of pasture seems to make amends for its poverty.

From Faro to Duina, Sept. 15, pass over an extensive plain, bounded by many small hills, oddly arranged; some of them detached and single, and all seem composed of sand collected in that form, having the plain for their general base. At Duina found a few plots of barley, which they are now cutting down, though green, as despairing of its ripening. The thermometer, at 6 o'clock in the morning below the freezing point, and the ground partially covered with snow.

Road to Chalu, Sept. 16. Continue on the plain; find 3 springs forcing their way through the ground with violence, and giving rise to a lake many miles in extent, stored with millions of water-fowl and excellent fish. Of the first saw the cygnus, solan geese, many kinds of ducks, pintados, cranes, and gulls of different sorts. The springs of this lake are in great reputation for the cure of most diseases. I examined the water, and found it contain a portion of alum with the selenitic earth. On the banks of the lake I found a crystallization, which proves to be an alkaline salt; it is used by the natives for washing, and answers the purpose as well as pot-ash. The pasture which is impregnated with this salt is greedily sought after by sheep and goats, and proves excellent food for them. The hills are chiefly composed of sand incrusted by the inclemency of the weather and violent winds, seeming at first view composed of freestone.

Road to Simadar, Sept. 17. Pass a lake still more considerable than the former, with which it communicates by a narrow stream, about 3 miles long. There never was a more barren or unpromising soil; little turf, grass, or vegetation of any sort, except near the lake. See a few huts, mostly in ruins and deserted. The only grain in this part of the country is barley, which they are cutting down every where green. Pass 2 springs, one of them slightly impregnated with alum. They form the principal source of a river, which empties itself in the Burrampooter near Tissoolumboo. The wind from the eastward of south is now the coldest and most piercing; passing over the snowy mountains and dry sandy desart before described, it comes divested of all vapour or moisture, and produces the same effect as the hot dry winds in more southerly situations. Mahogany boxes and furniture, that had withstood the Bengal climate for years, were warped with considerable fissures, and rendered useless. The

natives say, a direct exposure to these winds occasions the loss of their fore-teeth; and our faithful guide ascribed that defect in himself to this cause. We escaped with loss of the skin from the greatest part of our faces.

Road to Seluh, Sept. 18. Near our road to-day found a hot-well, much frequented by people with venereal complaints, rheumatism, and all cutaneous diseases. They do not drink the water, but use it as a bath. The thermometer, when immersed in the water, rose from 40° to 88°. It has a strong sulphureous smell, and contains a portion of hepar sulphuris. Exposure to air deprives it, as most other mineral wells, of much of its property.

Road to Takui, Sept. 19. Pass some fields of barley and pease, and get into a milder climate. Find to-day a great variety of stone and rock, some containing copper, and others, a very pure rock crystal, regularly crystallized, with 6 unequal sides. The rock crystal is of different sizes and degrees of purity, but of one form. Find some flint and granite, several springs of water impregnated with iron, and nearly of the same temperature with the atmosphere. See a few ill thriving willows planted near the habitations, and which are the only trees to be met with.

Road to Tissoolumboo, Sept. 20, 21, and 22. The remaining part of our journey is over a more fertile soil, enjoying a milder climate. Some very good fields of wheat, barley, and pease; many pleasant villages and distant houses, less sand and more rock, part slaty, and much of it a very good sort of flint. The soil in the valley a light coloured clay and sand. They are every where employed in cutting down their crop. What a happy climate! The sky serene and clear, without a cloud; and so confident are they of the continuance of this weather, that their crop is thrown together in a convenient part of the field, without any cover, to remain till they can find time to thresh it out. Before we reached Tissoolumboo found some elms and ash trees.

The hills in Thibet have, from their general appearance, strong marks of containing those fossils that are inimical to vegation; such are most of the ores of metal and pyritical matter. The country, properly explored, promises better than any I have seen to gratify the curiosity of a philosopher, and reward the labours of a mineralist. Accident, more than a spirit of enterprise and inquiry, has already discovered the presence of many valuable ores and minerals in Thibet. The first in this list is deservedly gold. They find it in large quantities, and frequently very pure. In the form of gold-dust it is found in the beds of rivers, and at their several bendings, generally attached to small pieces of stone, with every appearance of its having been part of a larger mass. They find it sometimes in large masses, lumps, and irregular veins; the adhering stone is generally flint or quartz, and I have sometimes seen a half-formed, impure sort of precious stone in the mass. By a common process for the purification of gold,

I extracted 12 per cent. of refuse from some gold-dust, and on examination found it to be sand and filings of iron, which last was not likely to have been with it in its native state, but probably employed for the purpose of adulteration. Two days journey from Tissoolumboo there is a lead mine. The ore is much the same as that found in Derbyshire, mineralized by sulphur, and the metal obtained by the very simple operation of fusion alone. Most lead contains a portion of silver, and some in the proportion to make it an object to work the lead ore for the sake of the silver. Cinnabar, containing a large proportion of quicksilver, is found in Thibet, and might be advantageously employed for the purpose of extracting this metal. The process is simple, by distillation; but to carry it on in the great would require more fuel than the country can well supply. I have seen ores and loose stones containing copper, and have not a doubt of its being to be found in great abundance in the country. Iron is more frequently to be met with in Boutan than Thibet; and, were it more common, the difficulty of procuring proper fuel for smelting the less valuable ores, must prove an insuperable objection to the working them. The dung of animals is the only substitute they have for fire-wood, and with that alone they will never be able to excite a degree of heat sufficiently intense for such purposes. Thus situated, the most valuable discovery for them would be that of a coal mine. In some parts of China bordering on Thibet, coal is found and used as fuel.

Tincal, the nature and production of which we have only hitherto been able to guess at, is now well known, and Thibet, from whence we are supplied, contains it in inexhaustible quantities. It is a fossil brought to market in the state it is dug out of the lake, and afterwards refined into borax by ourselves. Rock salt is likewise found in great abundance in Thibet. The lake, from whence tincal and rock salt are collected, is about 15 days journey from Tissoolumboo, and to the northward of it. It is encompassed on all sides by rocky hills, without any brooks or rivulets near at hand; but its waters are supplied by springs, which being saltish to the taste are not used by the natives. The tincal is deposited or formed in the bed of the lake; and those who go to collect it, dig it up in large masses, which they afterwards break into small pieces for the convenience of carriage, exposing it to the air to dry. Though tincal has been collected from this lake for a great length of time, the quantity is not perceptibly diminished; and as the cavities made by digging it soon wear out or fill up, it is an opinion with the people, that the formation of fresh tincal is going on. They have never yet met with it in dry ground or high situations, but it is found in the shallowest depths, and the borders of the lake, which deepening gradually from the edges towards the centre contains too much water to admit of their searching for the tincal conveniently; but from the deepest parts they bring up rock salt, which is not to be found in the shallows, or near the bank. The

waters of the lake rise and fall very little, being supplied by a constant and unvarying source, neither augmented by the influx of any current, nor diminished by any stream running from it. The lake is at least 20 miles in circumference, and standing in a very bleak situation is frozen for a great part of the year. The people employed in collecting these salts are obliged to quit their labour so early as October, on account of the ice. Tincal is used in Thibet for soldering and to promote the fusion of gold and silver. Rock salt is universally used for all domestic purposes in Thibet, Boutan, and Naphaul.

The thermometer at Tissoolumboo during the month of October was, on an average, at 8 o'clock in the morning 38°, at noon 46°, and 6 o'clock in the evening 42°. The weather clear, cool, and pleasant, and the prevailing wind from the southward. During the month of November we had frosts morning and evening, a serene clear sky, not a cloud to be seen. The rays of the sun passing through a medium so little obscured had great influence. The thermometer was often below 30° in the morning, and seldom above 38° at noon in the shade; wind from the southward.

Of the diseases of this country, the first that attracts our notice, as we approach the foot of the hills, is a glandular swelling in the throat, which is known to prevail in similar situations in some parts of Europe, and generally ascribed to an impregnation of the water from snow. The disease being common at the foot of the Alps, and confined to a tract of country near these mountains, has first given rise to the idea of its being occasioned by snow water. If a general view of the disease, and situations where it is common, had been the subject of inquiry, or awakened the attention of any able practitioner, we should have been long since undeceived in this respect. On the coast of Greenland, the mountainous parts of Wales and Scotland, where melted snow must be continually passing into their rivers and streams, the disease is not known, though it is common in Derbyshire, and some other parts of England. Rungpore is about 100 miles from the foot of the hills, and much farther from the snow, yet the disease is as frequent there as in Boutan. In Thibet, where snow is never out of view, and the principal source of all their rivers and streams, the disease is not to be met with; but what puts the matter past a doubt, is the frequency of the disease on the coast of Sumatra, where snow is never to be found. On finding the vegetable productions of Boutan the same as those of the Alps in almost every instance, it occurred to me, that the disease might arise from an impregnation of the water by these plants, or the soil probably possessing similar qualities, the spontaneous productions of both countries, with very few exceptions, being so nearly alike. It however appears more probable, that the disease is endemial, proceeding from a peculiarity in the air of situations in the vicinity of mountains with such soil and vegetable productions. I am the more inclined

to think so, that I have universally found this disease most prevalent among the lower class of people, and those who are most exposed to the unguarded influence of the weather, and various changes that take place in the air of such situations. The primary cause in the atmosphere producing this effect is, perhaps, not more inexplicable than what we meet with in the low-lands of Essex and fens of Lincolnshire. An accurate analysis of the water used in common by the natives, where this disease is more or less frequent, and where it is not known in similar exposures, might throw some light on this subject.

This very extraordinary disease has been little attended to, from obvious reasons; it is unaccompanied with pain, seldom fatal, and generally confined to the poorer sort of people. The tumor is unsightly, and grows to a troublesome size, being often as large as a person's head. It is certainly not exaggerating to say, that 1 in 6 of the Rungpore district, and country of Boutan, has the disease. As those who labour most, and are the least protected from the changes of weather, are most subject to the disease, we universally find it in Boutan more common with the women than men. It generally appears in Boutan at the age of 13 or 14, and in Bengal at the age of 11 or 12; so that in both countries the disease shows itself about the age of puberty, I do not believe this disease has ever been removed, though a mercurial course seemed to check its progress, but did not prevent its advance after intermitting the use of mercury. An attention to the primary cause will first lead to a proper method of treating the disease; a change of situation for a short while, at that particular period when it appears, might be the means of preventing it.

The people of this happy climate are not exempt from the venereal disease, which seems to rage with unremitting fury in all climates, and proves the greatest scourge to the human race. It has been long a matter of doubt, whether this disease has ever been cured by any other specific than mercury and its different preparations. In defence of the opinion of other specifics being in use, it has always been urged, that the disease is frequent in many parts of the world, where it could not be supposed that they were acquainted with quicksilver, and the proper method of preparing it as a medicine. I must own, that I expected to have been able to have added one other specific for this disease to our list in the Materia medica, being informed that the disease was common, and their method of treating it successful; nor could I allow myself to think that they were acquainted with the method of preparing quicksilver, so as to render it a safe and efficacious medicine. In this, however, I was mistaken.

The disease seems in this country to make a more rapid progress, and rage with more violence, than in any other. This is to be accounted for from the grossness of their food and little attention to cleanliness. There is one preparation of mercury in common use with them, and made after the following

manner. A portion of alum, nitre, vermillion, and quicksilver, are placed in the bottom of an earthen pot, with a smaller one inverted put over the materials, and well luted to the bottom of the larger pot. Over the small one, and within the large one, the fuel is placed, and the fire continued for about 40 minutes. A certain quantity of fuel, carefully weighed out, is what regulates them with respect to the degree of heat, as they cannot see the materials during the operation. When the vessel is cool, the small inverted pot is taken off, and the materials collected for use. I attended the whole of the process, and examined the materials afterwards. The quicksilver had been acted on by the other ingredients, deprived of its metallic form, and rendered a safe and efficacious remedy.

A knowledge of chemistry has taught us a more certain method of rendering this valuable medicine active and efficacious; yet we find this preparation answering every good purpose, and by their guarded manner of exhibiting it perfectly safe. This powder is the basis of their pill, and often used in external application. The whole, when intimately mixed, formed a reddish powder, and was made into the form of pills by the addition of a plumb or date. Two or 3 pills taken twice a day generally bring on, about the 4th or 5th day, a spitting, which is encouraged by continuing the use of the pills for a day or 2 longer. As the salivation advances, they put a stick across the patient's mouth, in the form of a gag, and make it fast behind. This they say is done to promote the spitting, and prevent the loss of their teeth. They keep up the salivation for 10 or 12 days, during which time the patient is nourished with congee and other liquids. Part of this powder is often used externally by diffusing it in warm water, and washing sores and buboes. They disperse buboes frequently by poultices of turnip tops, in which they always put vermillion, and sometimes musk. Nitre, as a cooler, is very much used internally by them in this disease, and they strictly enjoin warmth and confinement during the slightest mercurial course. Buboes advanced to suppuration are opened by a lancet, with a large incision, which they do not allow to close before the hardness and tumor are gone. In short, I found very little room for improving their practice in this disease. I introduced the method of killing quicksilver with honey, gave them an opportunity of seeing it done, and had the satisfaction of finding it successfully used by themselves before we left the country.

This happy climate presents us with but little variety in their diseases. Coughs, colds, and rheumatism, are more frequent here than in Bengal. Fevers generally arise here from a temporary cause, are easily removed, and seldom prove fatal. The liver disease is occasionally to be met with, and complaints in the bowels are not unfrequent; but the grossness of their food, and uncleanliness of their persons, would in any other climate be the source of constant disease and sickness. They are ignorant, as we were not many years ago, of the proper

method of treating diseases of the liver and other viscera; this is probably the cause of the most obstinate and fatal disease to be met with in the country, I mean the dropsy. As the Rajah had ever been desirous of my aid and advice, and had directed his doctors to attend to my private instructions and practice, I endeavoured to introduce a more judicious method of treating those diseases by mercurial preparations. I had an opportunity of proving the advantage of this plan to their conviction in several instances, and of seeing them initiated in the practice.

The Rajah favoured me with above 70 specimens of the medicines in use with them. They have many sorts of stones and petrifactions saponaceous to the touch, which are employed as an external application in swellings and pains of the joints. They often remove such complaints, and violent head-achs, by fumigating the part affected with aromatic plants and flowers. They do not seek for any other means of information respecting the state of a patient than that of feeling the pulse; and they confidently say, that the seat of pain and disease is easily to be discovered, not so much from the frequency of the pulse as its vibratory motion. They feel the pulse at the wrist with their three fore-fingers, first of the right, and then of the left hand; after pressing more or less on the artery, and occasionally removing 1 or 2 of the fingers, they determine what the disease is. They do not eat any thing the day on which they take physic, but endeavour to make up the loss afterwards by eating more freely than before, and using such medicines as they think will occasion costiveness.

The many simples in use with them are from the vegetable kingdom, collected chiefly in Boutan. They are in general inoffensive and very mild in their operation. Carminatives and aromatics are given in coughs, colds, and affections of the breast. The centaury, coriander, carraway, and cinnamon, are of this sort. This last is with them the bark of the root of that species of laurus formerly mentioned as a native of this country. The bark from the root is in this plant the only part which partakes of the cinnamon taste; and I doubt very much if it could be distinguished by the best judges from what we call the true cinnamon. The bark, leaves, berries, and stalks, of many shrubs and trees, are in use with them, all in decoction. Some have much of the astringent bitter taste of our most valuable medicines, and are generally employed here with the same view, to strengthen the powers of digestion, and mend the general habit. Their principal purgative medicines are brought by the Chinese to Lassa. They had not any medicine that operated as a vomit, till I gave the Rajah some ipecacuanha, who made the first experiment with it on himself. In bleeding they have a great opinion of drawing the blood from a particular part. For head-achs they bleed in the neck; for pains in the arm and shoulder, in the cephalic vein; and of the breast or side, in the median; and if in the belly, they bleed in the basilic vein.

They think pains of the lower extremity are best removed by bleeding in the ancle. They have a great prejudice against bleeding in cold weather; nor is any urgency or violent symptom thought at that time a sufficient reason for doing it.

They have their lucky and unlucky days for operating or taking any medicine; but I have known them get the better of this prejudice, and be prevailed on. Cupping is much practised by them; a horn, about the size of a cupping glass, is applied to the part, and by a small aperture at the other end they extract the air with their mouth. The part is afterwards scarified with a lancet. This is often done on the back; and in pain and swelling of the knee it is held as a sovereign remedy. I have often admired their dexterity in operating with bad instruments. Mr. Hamilton gave them some lancets, and they have since endeavoured, with some success, to make them of that form. They were very thankful for the few I could spare them. In fevers they use the kuthullega nut, well known in Bengal as an efficacious medicine. They endeavour to cure the dropsy by external applications, and giving a compounded medicine made up of above 30 different ingredients: they seldom or never succeed in effecting a cure of this disease. I explained to the Rajah the operation of tapping, and showed him the instrument with which it was done. He very earnestly expressed a desire that I should perform the operation, and wished much for a proper subject; such a one did not occur while I remained, and perhaps it was as well both for the Rajah's patients and my own credit; for after having seen it once done, he would not have hesitated about a repetition of the operation. Gravelish complaints and the stone in the bladder are probably diseases unknown here.

The small-pox, when it appears among them, is a disease that strikes them with too much terror and consternation to admit of their treating it properly. Their attention is not employed in saving the lives of the infected, but in preserving themselves from the disease. All communication with the infected is strictly forbidden, even at the risk of their being starved, and the house or village is afterwards erased. A promiscuous and free intercourse with their neighbours not being allowed, the disease is very seldom to be met with, and its progress always checked by the vigilance and terror of the natives. Few in the country have had the disease. Inoculation, if ever introduced, must be very general to prevent the devastation that would be made by the infection in the natural way; and where there could not be any choice in the subject fit to receive the disease, many must fall a sacrifice to it. The present Rajah of Thibet was inoculated, with some of his followers, when in China with the late Tishoo Lama. The hot bath is used in many disorders, particularly in complaints of the bowels and cutaneous eruptions. The hot wells of Thibet are resorted to by thousands. In Boutan they substitute water warmed by hot stones thrown into it. In Thibet the natives are more subject to sore eyes and blindness than

in Boutan. The high winds, sandy soil, and glare from the reflection of the sun, both from the snow and sand, account for this. I have dwelt long on this subject, because I think the knowledge and observations of these people on the diseases of their country, with their medical practice, keep pace with a refinement and state of civilization, which struck me with wonder, and no doubt will give rise to much curious speculation, when known to be the manners of a people holding so little intercourse with what we term civilized nations.

Dec. 1. Left Tishoolumbo, and found the cold increase every day as we advanced to the southward, most of the running waters frozen, and the pools covered with ice strong enough to carry. Our thermometer having only the scale as low as 16°, we could not precisely determine the degree of cold, the quicksilver being under that every morning. The frost is certainly never so intense in Great-Britain. On our return to the lakes the 14th, we found them deserted by the water fowl, and were informed that they had been one solid piece of ice since the 10th of November. Here we resumed our amusement of skating, to the great astonishment of the natives and Bengal servants.

On the 17th we re-entered Boutan, and in 6 days more arrived at Punukha by Paraghon. No snow or frost to be met with in Boutan, except towards the tops of their highest mountains; the thermometer rising to 36° in the morning, and 48° at noon. Took leave of the Debe Rajah, and on the 12th arrived at Buxaduar. Calcutta, Feb. 17, 1784.

As Lac is the produce of, and a staple article of commerce in Assam, a country bordering on and much connected with Thibet, some account of it may not be an improper supplement to the above remarks. Lac is, strictly speaking, neither a gummy nor resinous substance, though it has some properties in common to both. Gums are soluble in water, and resins in spirits; lac admits of a very difficult union with either, without the mediation of some other agent. Lac is known in Europe by the different appellations of stick lac, seed lac, and shell lac. The first is the lac in pretty considerable lumps, with much of the woody parts of the branches on which it is formed adhering to it. Seed lac is only the stick lac broke into small pieces, garbled, and appearing in a granulated form. Shell lac is the purified lac, by a very simple process to be mentioned afterward.

Many vague and unauthenticated reports concerning lac have reached the public; and though among the multiplicity of accounts the true history of this substance has been nearly hit on, little credit is given in Europe to any description of it hitherto published. My observations, as far as they go, are the result of what I have seen, from the lac on the tree, the progress of the insect now in my custody, and the information of a gentleman residing at Goalpara on the borders of Assam, who is perfectly versant in the method of breeding the insect, inviting it to the tree, collecting the lac from the branches, and forming

it into shell lac, in which state much of it is received from Assam, and exported to Europe for various great and useful purposes. The tree on which this fly most commonly generates is known in Bengal by the name of the Biher tree, and is a species of the rhamnus. The fly is nourished by the tree, and there deposits its eggs, which nature has provided it with the means of defending from external injury by a collection of this lac, evidently serving the twofold purpose of a nidus and covering to the ovum and insect in its first stage, and food for the maggot in its more advanced state. The lac is formed into complete cells, finished with as much regularity and art as a honey-comb, but differently arranged. The flies are invited to deposit their eggs on the branches of the tree, by besmearing them with some of the fresh lac steeped in water, which attracts the fly, and gives a better and larger crop. The lac is collected twice a year, in the months of February and August.

I have examined the egg of the fly with a very good microscope; it is of a very pure red, perfectly transparent, except in the centre, where are evident marks of the embryo forming, and opaque ramifications passing off from the body of it. The egg is perfectly oval, and about the size of an ant's egg. The maggot is about the 8th of an inch long, formed of 10 or 12 rings, with a small red head; when seen with a microscope, the parts of the head were easily distinguished, with 6 small specks on the breast, somewhat projecting, which seemed to be the incipient formation of the feet. This maggot is now in my custody, in the form of a nymph or crysalis, its annular coat forming a strong covering, from which it should issue forth a fly. I have never seen the fly, and cannot therefore describe it more fully, or determine its genus and species. I am promised a drawing of the insect in its different stages, and shall be able soon to add to a botanical description of the plant, a drawing of the branch, with the different parts of fructification and lac on it. The gentleman to whom I owe part of my information terms the lac the excrement of the insect. On a more minute investigation however, we may not find it more so than the wax or honey of the bee, or silk of the silk-worm. Nature has provided most insects with the means of secreting a substance which generally answers the twofold purpose of defending the embryo, and supplying nourishment to the insect from the time of its animation till able to wander abroad in quest of food. The fresh lac contains within its cells a liquid, sweetish to the taste, and of a fine red colour, miscible in water. The natives of Assam use it as a dye, and cotton dipped in this liquid makes afterwards a very good red ink.

The simple operation of purifying lac is practised as follows. It is broken into small pieces, and picked from the branches and sticks, when it is put into a sort of canvas bag of about 4 feet long, and not above 6 inches in circumference. Two of these bags are in constant use, and each of them held by 2 men. The

bag is placed over a fire, and frequently turned till the lac is liquid enough to pass through its pores, when it is taken off the fire, and squeezed by 2 men in different directions, dragging it along the convex part of a plantain-tree prepared for the purpose; while this is doing, the other bag is heating, to be treated in the same way. The mucilaginous and smooth surface of the plantain-tree seems peculiarly well adapted for preventing the adhesion of the heated lac, and giving it the form which enhances its value so much. The degree of pressure on the plantain-tree regulates the thickness of the shell, and the quality of the bag determines its fineness and transparency. They have learned of late that the lac, which is thicker in the shell than it used to be, is most prized in Europe. Assam furnishes us with the greatest quantity of lac in use; and it may not be generally known, that the tree on which they produce the best and largest quantity of lac, is not uncommon in Bengal, and might be employed in propagating the fly, and cultivating the lac, to great advantage. The small quantity of lac collected in these provinces affords a precarious and uncertain crop, because not attended to. Some attention at particular seasons is necessary to invite the fly to the tree; and collecting the whole of the lac with too great an avidity, where the insect is not very generally to be met with, may annihilate the breed.

The best method of cultivating the tree, and preserving the insect, being properly understood in Bengal, would secure to the Coss possessions the benefit arising from the sale of a lucrative article, in great demand and of extensive use.

A Meteorological Journal kept at the Apartments of the Royal Society, for the Year 1788, by Order of the President and Council. p. 113.

A synopsis for the whole year is as follows:

1788.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Inches.
	°	°	°	°	°	°	Inches.	Inches.	Inches.	
January	48	26	39.7	56	49	52.7	30.70	28.89	29.97	0.439
February ..	50	29	41.3	56	50	52.7	30.21	28.65	29.68	1.461
March	59	28	40.8	59	46	50.9	30.08	29.32	29.68	0.336
April......	68	40	52.6	63	55	51.8	30.48	29.50	30.07	0.607
May	80	49	60.0	73	59	62.8	30.34	29.58	30.04	0.497
June	80	52	62.3	70	61	64.1	30.22	29.49	29.94	3.275
July	77	55	63.7	69	64	65.9	30.22	29.73	29.99	1.620
August	77	53	63.4	71	64	66.0	30.45	29.22	29.95	2.699
September..	74	45	58.6	67	59	63.7	30.25	29.37	29.86	3.345
October....	67	33	51.4	62	54	59.4	30.55	29.64	30.32	0.103
November..	58	27	42.9	61	47	56.4	30.50	29.61	30.11	0.510
December ..	46	18	30.9	51	39	45.2	30.33	29.50	29.92	0.000
Whole year			50.6			57.6			29.96	14.892

XI. *Experiments on the Phlogistication of Spirit of Nitre. By the Rev. J. Priestley, LL.D., F.R.S.* p. 139.

In my former experiments, vol. 4, p. 2, I found that the colourless acid became smoking, or orange-coloured, and emitted orange-coloured vapours, on being exposed to heat in long glass tubes, hermetically sealed; and I then concluded, that this effect was produced by the action of heat, evolving, as it were, the phlogiston previously contained in the acid. Afterwards, having found that it was not heat, but light only, that was capable of giving colour to spirit of nitre, contained in phials with ground stoppers, in the course of several days; and that in this case the effect was produced by the action of light on the vapour, which gradually imparted its colour to the liquor on which it was incumbent (see vol. 5, p. 342,) I was led to suspect, that as the glass tubes, in which I had formerly exposed this acid to the action of heat, were only held near to a fire, in the day-light, or candle-light, it might have been this light, which in these circumstances had, at least in part, contributed to produce the effect.

In order to ascertain whether the light had had any influence in this case, I now put the colourless spirit of nitre into long glass tubes, like those which I had used before, and also sealed them hermetically, as I had done the others; but, instead of exposing them to heat in the open air, from which light could not be excluded, I now shut them up in gun barrels, closed with metal screws, so that it was impossible for any particle of light to have access to them; and I then placed one end of the barrels so near a fire as was sufficient to make the liquor contained in the tube to boil, which I could easily distinguish by the sound it yielded. The consequence was, that in a short time the acid became as highly coloured as ever it had been when exposed to heat without the gun barrel. It was evident therefore, that it had been mere heat, and not light, which had been the means of giving this colour to the acid, and which has been usually termed phlogisticating it.

When I made the former experiments, I had no suspicion that the air contained in the tube had any concern in the result of them; and, in those which I made in the phials in a moderate heat, I found that the acid received its colour when the best vacuum that I could make with an air pump was over it. My friend Mr. Kirwan, however, having always suspected, that the air was a principal agent in the business, I at this time gave particular attention to this circumstance; supposing that, if any part of the common air had been imbibed, it must have been the phlogisticated, and that it was the phlogiston from this kind of air which had phlogisticated the acid. The real result however was not so much in favour of this supposition as I had expected; for the principal effect of the process was the emission of dephlogisticated air, so that the acid seems to become what we call phlogisticated, by parting with this ingredient in its composition.

I put a small quantity of the colourless acid into a long glass tube, which, besides the acid, would have contained 1.23 ounce measures of common air, but that

the vapour of the acid excluded about $\frac{1}{10}$ of the quantity. Having sealed the tube hermetically, I shut it up in a gun barrel, in the manner above mentioned, and exposed it to a boiling heat for several hours, and then opening it under water, there came out of it 2.03 ounces measures of air, very turbid and white; and when examined, it appeared to be of the standard of 1.02, with 2 equal measures of nitrous air; when with 1 measure of the same nitrous air the standard of the common air was 1.07.

In order to exclude all air from the contact of the acid, I made a quantity of it to boil in the tube, and when the vapour had expelled all the air, I sealed it hermetically, in the manner in which water hammers are made; and then exposing it to heat, found that it acquired as high a colour as when air had been confined along with it; so that it is evident, that air is not necessary to this effect. When the tube was opened under water, a quantity of dephlogisticated air rushed out, exceedingly white as before; but when I examined it, I found it to be of the standard of only 0.66. When this impurity is considered, it will appear, that when much air is yielded in this process, some phlogisticated air may have been imbibed, though, computing in the manner above mentioned, the phlogisticated air after the process should be in greater quantity than was contained in the tube before it, as was the case in the following experiment. In a glass tube which, besides the acid, contained 1.13 oz. m. of common air, I exposed colourless spirit of nitre to heat till it became of a deep orange colour; and when it was opened under water, there came out of it 2.83 oz. m. of air exceedingly turbid, of the standard of 0.66, with 2 equal quantities of nitrous air, when that of the common air, with one equal quantity of nitrous air, was 1.07. Computing in the manner above mentioned, there was in the tube before the process 0.7477 oz. m. of phlogisticated air, and after the process 0.8792 oz. m. But the dephlogisticated air, amounting to 1.7 oz. m. being of the standard of 0.66, will be found to contain 0.374 oz. m. of phlogisticated air, which being deducted from 0.8792, there will remain only 0.5052 oz. m. which is considerably less than 0.7477 oz. m.

Having repeatedly observed, that the acid became coloured in consequence of being exposed to heat in contact with any kind of air whatever, I exposed at the same time, and in the same circumstances, 3 equal quantities of the same colourless spirit of nitre, in 3 nearly equal tubes, one containing dephlogisticated, another phlogisticated, and a 3d inflammable air; that, if there should be any difference in the colouring of the acid in these cases, it might be the more easily perceived. But though I gave all the attention that I could, I did not perceive that there was any difference, except what arose from some of the tubes being placed a little nearer the fire than the rest; and, by changing their places, the colour was at length the very same in them all.

As the spirit of nitre can be rendered smoking, or phlogisticated, by the mere

expulsion of dephlogisticated air, it is evident that it contains 2 principles in close affinity with each other, and that nothing is necessary to render either of them conspicuous besides the absence of the other.

It is also natural to suppose that, for the same reason that the dephlogisticating principle, as it may be called, is expelled, the phlogisticating principle should enter; so that the purification of the air in contact with the acid may be a necessary consequence of the expulsion of the pure air contained in it, the whole tending, as it were, to an equilibrium in this respect. It is therefore by no means difficult to conceive, that phlogiston should be extracted from the contiguous air at the same time that the dephlogisticated air not pure, that is containing a mixture of phlogisticated air, is driven out of it; for the acid always containing phlogistion, whatever air is contained in it, and expelled from it, may necessarily contain phlogiston or phlogisticated air; but the purer air may be emitted, and the less pure air be imbibed, till the whole come to be of the same quality. It may however perhaps follow from the emission of impure dephlogisticated air, and the imbibing of phlogisticated air at the same time, that the former does not consist of dephlogisticated and phlogisticated air loosely mixed, but of some intimate union of dephlogisticated air with phlogiston, though they may be separated by a mixture of nitrous air, and other processes, in the very same manner as dephlogisticated air may be separated from a loose mixture of phlogisticated air. It is evident from these experiments, that a red heat is not necessary to the conversion of nitrous acid into pure air, though this process, as appeared by my former experiments, produces this effect most quickly and effectually.

I cannot help considering the experiments above recited to be favourable to the doctrine of the phlogiston, and unfavourable to that of the decomposition of water, though not decisively so; for since the red vapour of spirit of nitre unquestionably contains the same principle that has been termed phlogiston, or the principal element in the constitution of inflammable air, and according to the antiphlogistians this is one constituent part of water, they must suppose, that the water in this acid is decomposed by a much more moderate heat than in most other cases. In general I believe they have thought a red heat to be necessary for this purpose. It is evident, that the conversion of water into steam by boiling, or by any heat that can be given to it under the strongest pressure, has no tendency whatever to decompose it. But if the mere boiling of water in nitrous acid could produce this effect, I do not see why the same should not be the case when water alone is boiled. I think it will also be more difficult to explain the purification of the incumbent atmospherical air on the antiphlogistic than on the phl gistic hypothesis, whatever be the constitution of phlogisticated air.

As, in the experiments above mentioned, heat without light gives colour to the nitrous acid, and the reflection or refraction of light is always attended with heat, it may perhaps be heat universally that is the means of imparting this colour, though the mode of its operation be at present unknown. And in these experiments, as well as the former, it is the vapour that first receives the colour, and imparts it to the liquid when it is sufficiently cold to receive it. The rushing out of a quantity of turbid white air from a transparent tube, quite cold, is a striking phænomenon in these experiments. It may be worth while to examine of what it is that this remarkable cloudiness of the air consists. There is the same appearance in the rapid production of any kind of air, which is perfectly transparent, as it passes along the glass tube through which it is transmitted, till it comes into contact with the water in which it is received.

For further observations, we may refer to Dr. P's separate publications on this subject.

XII. Observations on a Comet. By Wm. Herschel, LL. D., F. R. S. p. 151.

December 21, 1788, about 8 o'clock, I viewed the comet which my sister had a little while before pointed out to me with her small Newtonian sweeper. In my instrument, which was a 10 feet reflector, it had the appearance of a considerably bright nebula; of an irregular, round form; very gradually brighter in the middle; and about 5 or 6 minutes in diameter. The situation was low, and not very proper for instruments with high powers. Dec. 22, about half after 5 in the morning, I viewed it again, and perceived that it had moved apparently in a direction nearly towards δ Lyræ. I had been engaged all night with the 20 feet instrument, so that there had been no leisure to prepare my apparatus for taking the place of the comet; but in the evening of the same day, I took its situation 3 times, as follows: viz. Dec. 22, sidereal time,

At	$23^h\ 42^m\ 19^s$....	$23^h\ 52^m\ 52^s$....	$0^h\ 6^m\ 35^s$	comet passed the wire.
At	23 49 24....	23 59 58....	0 13 40	β Lyræ passed it.
Diff.	00 7 5....	00 7 6....	0 7 5	

I found in every observation the small star which accompanies β Lyræ, exactly in the parallel of the comet. These transits were taken with a 10 feet reflector; and the difference in right ascension, I should suppose, may be depended on to within a second of time. The determination also of the parallel can hardly err so much as 15″ of a degree. This, and several evenings afterwards, I viewed the comet again with such powers as its diluted light would permit, but could not perceive any sort of nucleus, which, had it been a single second in diameter, I think, could not well have escaped me. This circumstance seems to be of some consequence to those who turn their thoughts on the investigation of the nature of comets; especially as I have also formerly made the same remark on one of

the comets discovered by M. Mechain in 1787, a former one of my sister's in 1786, and one of Mr. Pigott's in 1783; in neither of which any defined, solid nucleus could be perceived.

XIII. Indications of Spring, observed by Robert Marsham, Esq., F. R. S., of Stratton in Norfolk. Latitude 52° 45′.

This is merely a register, for each year from 1736 till 1788, of the earliest date, month and day in each year, when the following circumstances occurred or took place, each being arranged in a separate column; viz. snowdrop flower, thrush sings, hawthorn leaf, hawthorn flower, frogs and toads croak, sycamore leaf, birch leaf, elm leaf, mountain-ash leaf, oak leaf, beech leaf, horse-chesnut leaf, chesnut leaf, hornbeam leaf, ash leaf, ringdoves coo, rooks build, young rooks, swallows appear, cuckoo sings, nightingale sings, churn owl sings, yellow butterfly appears, turnip in flower, lime leaf, maple leaf, wood anemone flower.

XIV. Account of a Monster of the human Species, in two Letters; one from Baron Reichel to Sir Jos. Banks, Bart., and the other from Mr. Jas. Anderson to Baron Reichel. p. 157.

To Sir Joseph Banks, Bart. *Fort St. George, Feb.* 28, 1788.

Sir,—I have the pleasure to transmit to you the portrait of a Gentoo boy, an astonishing living subject, who being sent to me by a friend of mine residing in the environs of the native place of the boy, I made 2 drawings representing the alternate attitudes in which he can place half the body of his little brother, who adheres to his breast. Peruntaloo is a handsome well-made lad, possessing every due faculty of mind and body, rather more sagacious, and with a superior share of understanding, than young men in general of his age. In addition to the inclosed anatomical description of the boy by Mr. Anderson, you will observe in the drawings 2 circular dotted lines, about the lower part of the loins of the semi-monster. During the several sittings I had of Peruntaloo, I observed an internal motion about these parts rather more conspicuous than any other of the body; and on questioning the youth, he showed me, that by retaining his breath, he could force a current of air into them, so as to swell the parts like 2 blown-up bladders, with a rumbling noise at the time of action. Whether there is a connection with the lungs of Peruntaloo is a question I cannot venture to determine; Mr. Anderson however thinks it well worth my mentioning this observation. The erection of the little penis in the semi-monster, and the command Peruntaloo has of discharging the urine through it, are perfectly ascertained.

I am, &c. T. Reichel.

To Baron Reichel. *Fort St. George, Feb.* 25, 1788.

Sir,—As you mean to send the elegant drawing of Peruntaloo to Sir Joseph

Banks, you may acquaint him from me, that the little brother is suspended by the os pubis; an elongation of the sword-like cartilage of Peruntaloo having anastomosed with that bone at the symphysis. The lower orifice of the stomach seems to lie in the sac or cylindrical cavity between the two brothers on the right side, and what may be reckoned the right hypochondre of the little one, as that part is tumid and full after eating. The alimentary canal must be common to both, as the anus of the little one is imperforate. There is a bladder of urine distinctly perceived, which occupies the left side of the sac, or left hypochondre of the monster. Besides which, there remain only the sacrum, ossa innominata, and lower extremities perfect.

Peruntaloo says he has as complete a sense of feeling with every part of the body of his little brother as of his own proper body, and this may account for the erections you saw, and making water distinctly; but this volition does not extend to the legs or feet, which are cold in comparison with the rest.

I am, &c. JAMES ANDERSON.

XV. A Supplementary Letter on the Identity of the Species of the Dog, Wolf, and Jackal; from John Hunter, Esq. F. R. S. p. 160.

In the year 1787 I had the honour of presenting to the R. S. a paper to prove the wolf, the jackal, and the dog to be of the same species. But as the complete proof of the wolf being a dog, which consisted in the half-bred puppy breeding again, had not been under my own inspection, though sufficiently well authenticated, I saved a female of one of the half-bred puppies, mentioned in that paper; in hopes of being myself a witness of the fact; but when the period of impregnation arrived, we unluckily missed that opportunity. However, another half-bred puppy has had young, which is equally satisfactory to me as if my own had bred. John Symmons, Esq. of Milbank, has had a female wolf in his possession for some time, which was lined by a dog, and brought forth several puppies. This was a very short time after the brood had been produced by Mr. Gough's wolf, the subject of my former paper, therefore the puppies were nearly of an age with mine. These puppies Mr. Symmons has reared; only one of them was a female, and she had much more of the mother or wolf in her than any of the rest of the same litter. I communicated my wish to Mr. Symmons, that either his puppy or mine should prove the fact to our own knowledge; which he immediately, with great readiness, acceded to. On the 16th, 17th, and 18th of December, 1788, this bitch was lined by a dog, and on the 18th of February she brought 8 puppies, all of which she now rears. If we reckon from the 16th of December, she went 64 days; but if we reckon from the 17th, the mean time, then it is 63 days, which is the usual time for a bitch to go with pup. These puppies are the 2d remove from the wolf and dog,

similar to that given by my Lord Clanbrassil to the Earl of Pembroke, which bred again. (See Philos. Trans. vol. 77, p. 255.) It would have proved the same fact if she had been lined by either a wolf, a dog, or one of the males of her own litter.

I may just remark here, that the wolf seems to have only one time in the year for impregnation natural to her, and that is in the month of December; for every time Mr. Gough's wolf has been in heat was in this month, and it proves to be the same month in which Mr. Symmons's wolf was in heat; for his half-bred wolf is nearly of the same age with mine, and the time she was in heat was also the same with that of her own mother, and the present brood corresponds in time with the brood of Mr. Gough's wolf.

XVI. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon in Rutland, for 1788. By T. Barker, Esq. Also of the Rain in Hampshire and Surrey, p. 162.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.					Hampshire.	
		Highest.	Lowest.	Mean.	Hig.	Low	Mean	Hig.	Low	Mean	Lyndon.	Surry. S. Lamb.	Selbourn	Fyfield.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	30.13	28.37	29.50	44	34	40	45	23½	36	0.970	0.68	1.60	1.10
	Aftern.				44	35	40½	49	30	41				
Feb.	Morn.	29.77	28.25	29.14	45	35	40½	44	27½	36	2.667	2.09	3.37	2. 6
	Aftern.				45	37	41	48	30	42				
Mar.	Morn.	29.65	28.84	29.23	51	34	40	50	22	35	1.072	0.64	1.31	1.36
	Aftern.				52½	35½	41	63	31	43				
Apr.	Morn.	30.02	28.94	29.59	56	42½	50	54	35	45½	0.588	0.47	0.61	0.50
	Aftern.				60	43½	51	68½	40	56				
May	Morn.	29.92	29.19	29.60	68½	51	58	64	43½	52½	1.517	0.81	0.76	0.28
	Aftern.				72	53	60	82	51	66				
June	Morn.	29.85	29.10	29.52	65½	56	60	64½	50	56	0.608	1.94	1.27	1.36
	Aftern.				69	57½	61	82½	58	67				
July	Morn.	29.78	29.21	29.52	67½	58	62	70½	51	59	1.795	1.84	3.58	1.81
	Aftern.				70	59½	63½	83	58	72				
Aug.	Morn.	30.01	28.88	29.49	68	57½	61½	64	54	56	2.780	4.30	3.22	3.40
	Aftern.				70½	59	63	77	62	68				
Sept.	Morn.	29.80	29.00	29.40	66	52½	58½	61½	42	52	2.430	3.81	5.71	3.78
	Aftern.				66½	53	59	75½	50	63				
Oct.	Morn.	30.15	29.15	29.68	59	46	52	57	32	46	1.412	0.08	0.00	0.03
	Aftern.				60	47	53	66	45	54½				
Nov.	Morn.	30.01	29.06	29.62	53	37	45	51½	25½	39	0.453	0.62	0.86	0.74
	Aftern.				53	37½	46	58	31	45				
Dec.	Morn.	29.85	29.12	29.47	39	27	34	40½	15	27	0.890	0.00	0.21	0.42
	Aftern.				40½	28	34½	44½	22½	31½				
Means and sums				29.48			50½			49¾	17.182	17.28	22.50	16.84

XVII. On the Method of Correspondent Values, &c. By Edw. Waring, M.D., F.R.S. p. 166.

§ 1.—1. In the year 1762 I published a method of finding when 2 roots of a

given equation $x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + \&c. = 0$ are equal, by finding the common divisors of the 2 quantities $a^n - pa^{n-1} + qa^{n-2} - \&c.$, and $na^{n-1} - (n-1)pa^{n-2} + (n-2)qa^{n-3} - \&c.$, and observed if they admitted only one simple divisor, $a - A$, then 2 roots only were equal; if a quadratic, $a^2 - Aa + B$, then 2 roots of the equation became twice equal; if a cubic, $a^3 - Aa^2 + Ba - C$, then 2 roots became thrice equal; and so on: or, to express in more general terms what follows from the same principles, if the common divisor be $\overline{a-b}^r \times \overline{a-c}^s \times \overline{a-d}^t \times \&c.$, then $r+1$ roots of the given equation will be b, $s+1$ roots will be c, $t+1$ will be d, &c.; and it immediately follows, from the principles delivered in the 2d edition of the same book, published in 1770, that to find when $r+1$, $v+1$, $t+1$, &c. roots are respectively equal, requires $r+s+t$, &c. equations of condition, which are deducible from the well-known method of finding the common divisors of 2 quantities in this case of $a^n - pa^{n-1} + qa^{n-2} - \&c.$, $na^{n-1} - (n-1)pa^{n-2} + (n-2)qa^{n-3} - \&c.$ of the terms of their remainders, &c.

In the book above-mentioned the equations of condition are given, which discover when 2 roots are equal in the equations $x^3 - px^2 + qx - r = 0$, $x^4 + qx^2 - rx + s = 0$, $x^5 + qx^3 - rx^2 + sx - t = 0$, in the 2 latter equations the 2d term is wanting, which may easily be exterminated; but it may as easily be restored by substituting for q, r, s, &c. in the equation of condition found the quantities resulting from the common transformation of equations to destroy the 2d term.

2. Another rule contained in the same book is the substitution of the roots of the equation $na^{n-1} - (n-1)pa^{n-2} + (n-2)qa^{n-3} - \&c. = 0$ respectively for a in the quantity $a^n - pa^{n-1} + qa^{n-2} - \&c.$, and multiplication of of all the quantities resulting into each other; their content will give the equation of condition, when 2 roots are equal. Mr. Hudde first discovered, that if the successive terms of the given equation are multiplied into an arithmetical series, the resulting equation will contain one of any 2 equal roots, and m of the $m+1$ equal roots in the given equation.

3. If 3, 4, 5, .. r roots of the equation are equal, find a common divisor of 3, 4, 5, .. r of the subsequent quantities $a^n - pa^{n-1} + qa^{n-2} - \&c.$, $na^{n-1} - (n-1)pa^{n-2} + (n-2)qa^{n-3} - \&c.$, $n.(n-1)a^{n-2} - (n-1).(n-2)pa^{n-3} + (n-2).(n-3)qa^{n-4} - (n-3).(n-4)ra^{n-5} + \&c.$, $n.(n-1).(n-2)a^{n-3} - (n-1).(n-2).(n-3)pa^{n-4} + (n-2).(n-3).(n-4)qa^{n-5} - \&c.$, ... $n.(n-1).(n-2)..(n-r+2)a^{n-r+1} - (n-1).(n-2)..(n-r+1)pa^{n-r} + \&c.$; which will probably be best done by dividing all the preceding quantities by the quantity of the least dimension of a, and the divisor and all the remainders by that quantity which has the least dimensions among them; and so on: there will result 2, 3, 4, .. $r-1$ equations of condition; and in this case it is observed, in the before-mentioned book, that if the

common divisor be $a - A$, it will once only admit of 3, 4, 5, .. r equal roots; if it be a quadratic, then it will twice admit of those equal roots; and so on.

4. If the roots of the equation of the least dimensions be substituted for a in the remaining equations, and each of the resulting values of the same equation be multiplied into each other, there will result the $r - 1$ equations of condition: and the same may be deduced also from the several equations conjointly. The equations of conditions found by the first method, if the divisions were not properly instituted, may admit of more rational divisors than necessary, of which some are the equations of conditions required.

§ 2.—1. In the year 1776, I published in the Meditationes Analyticæ a new method of differences for the resolution of the following problem. Given the sums of a swiftly converging series $ax + bx^2 + cx^3 + dx^4 +$ &c., when the values of x are respectively π, ϱ, ς, &c.; to find the sum of the series when x is τ, that is, given $s\pi = a\pi + b\pi^2 + c\pi^3 + d\pi^4 +$ &c., $s\varrho = a\varrho + b\varrho^2 + c\varrho^3 +$ &c., $s\sigma = a\sigma + b\sigma^2 + c\sigma^3 +$ &c. &c.; to find $s\tau = a\tau + b\tau^2 + c\tau^3 +$ &c.

To resolve this problem I multiplied the quantities, $s\pi$, $s\varrho$, $s\sigma$, &c. respectively into unknown co-efficients α, β, γ, &c. and there resulted as in the margin; and then made the sum of each of the terms respectively equal to its correspondent term of the quantity $\tau a + \tau^2 b + \tau^3 c +$ &c., and consequently $\alpha\pi + \beta\varrho + \gamma\sigma +$ &c. $= \tau$, $\alpha\pi^2 + \beta\varrho^2 + \gamma\sigma^2 +$ &c. $= \tau^2$, $\alpha\pi^3 + \beta\varrho^3 + \gamma\sigma^3 +$ &c. $= \tau^3$, &c. I assumed as many equations of this kind as there were given values π, ϱ, σ, &c. of x; and consequently as many equations resulted as unknown quantities α, β, γ, &c.; whence, by the common resolution of simple equations, or more easily from differences, can be found the unknown quantities α, β, γ, &c., and thence the equation sought $\alpha \times s\pi + \beta \times s\varrho + \gamma \times s\sigma +$ &c. $= s\tau$ nearly.

$\alpha\pi a + \alpha\pi^2 b + \alpha\pi^3 c +$ &c.
$\beta\varrho a + \beta\varrho^2 b + \beta\varrho^3 c +$ &c.
$\gamma\sigma a + \gamma\sigma^2 b + \gamma\sigma^3 c +$ &c.
&c. &c. &c.

2. In the Meditationes are assumed for π, ϱ, σ, &c. the quantities p, $2p$, $3p$, $4p$, ... $n - 2p$, $n - 1p$, and np for τ; which, if substituted for their values in the preceding equations, will give $\alpha + 2\beta + 3\gamma + 4\delta +$ &c. $= n$, $\alpha + 4\beta + 9\gamma + 16\delta +$ &c. $= n^2$, $\alpha + 8\beta + 27\gamma +$ &c. $= n^3$, $\alpha + 16\beta + 81\gamma +$ &c. $= n^4$; and if the sums of the series $ax + bx^2 + cx^3 +$ &c. which respectively correspond to the values p, $2p$, $3p$, ... $n - 1p$ of x be s1, s2, s3, s4, ... s$(n-1)$, and the sum of the series $ax + bx^2 + cx^3 +$ &c. which corresponds to n value of x be sn; then will $sn = ns(n-1) - n \cdot \frac{n-1}{2} s(n-2) + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} s(n-3) \ldots \pm ns1$ nearly, which equation is given in the above-mentioned book.

3. The logarithm from the number, the arc from the sine, &c. are found by serieses of the formula $ax + bx^2 + cx^3 +$ &c.; and consequently this equation is applicable to them.

4. In the same book is assumed a series $ax^r + bx^{r+s} + cx^{r+2s} + dx^{r+3s} +$ &c. of a more general formula than the preceding, and in it for x substituted $\alpha, \beta, \gamma, \delta$, &c., m; and $s\alpha$, $s\beta$, $s\gamma$, $s\delta$, &c.; sm for the resulting sums, and thence deduced $sm =$

$$\frac{m^r \times m^s - \beta^s \cdot m^s - \gamma^s \cdot m^s - \delta^s \cdot \&c.}{\alpha^r \times \alpha^s - \beta^s \cdot \alpha^s - \gamma^s \cdot \alpha^s - \delta^s \cdot \&c.} \times s\alpha + \frac{m^r \times m^s - \alpha^s \cdot m^s - \gamma^s \cdot m^s - \delta^s \cdot \&c.}{\beta^r \times \beta^s - \alpha^s \cdot \beta^s - \gamma^s \cdot \beta^s - \delta^s \cdot \&c.} \times s\beta + \frac{m^r \times m^s - \alpha^s \cdot m^s - \beta^s \cdot m^s - \delta^s \cdot \&c.}{\gamma^r \times \gamma^s - \alpha^s \cdot \gamma^s - \beta^s \cdot \gamma^s - \delta^s \cdot \&c.} \times s\gamma + \frac{m^r \times m^s - \alpha^s \cdot m^s - \beta^s \cdot m^s - \gamma^s \times \&c.}{\delta^r \times \delta^s - \alpha^s \cdot \delta^s - \beta^s \cdot \delta^s - \gamma^s \cdot \&c.}$$

$\times\, s\delta +$ &c. nearly.

Cor. If for r and s be assumed respectively 1, the series becomes $ax + bx^2 + cx^3 +$ &c. of the same formula as the preceding: if $r = 0$ and $s = 1$, the series becomes $a + bx + cx^2 +$ &c. The latter case will be the same as the former, when one of the quantities α, substituted for x and its correspondent sum $s\alpha$, both become $= 0$, and the equation deduced in both cases the same.

5. If π, ϱ, σ, &c. respectively denote $r, r + p, r + 2p, \ldots r + (n-2)p, r + (n-1)p$, and $\tau = r + np$; and s, s1, s2, s3,.. s$(n-2)$, s$(n-1)$, be the sums either resulting from the series $ax + bx^2 + cx^3 +$ &c. or the series $A + ax + bx^2 + cx^3 +$ &c., which respectively correspond to the values r, $r + p$, $r + 2p$, &c. of x; and sn the sum of the same series which corresponds to the value $r + np$ of x; then will $sn = ns(n-1) - n \cdot \frac{n-1}{2} s(n-2) + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} s(n-3) - \ldots \pm n \cdot \frac{n-1}{2} s2 \mp ns1 \pm s$ nearly; this equation differs from the preceding by the last term s not vanishing; in the preceding case s became $= 0$, for it was the sum of the series $ax + bx^2 + cx^3 +$ &c. which corresponded to $x = 0$.

6. From the Meditationes it appears that $r^m - n \times (r \pm p)^m + n \cdot \frac{n-1}{2} (r \pm 2p)^m - n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} (r \pm 3p)^m +$ &c. to the end of the series $= 0$, if m is less than n, and m and n are whole numbers; but if $m = n$, then it will $= \pm 1 \cdot 2 \cdot 3 \cdot 4 \ldots n - 1 \cdot np^m$; whence it is manifest, that for the n first terms of the series $A + ax + bx^2 +$ &c. the equations are true; and for the $n - 1$ first terms of the series $ax + bx^2 + cx^3 +$ &c. and in the successive term of both the serieses, they will err by a quantity nearly $= \pm 1 \cdot 2 \cdot 3 \ldots n \times p^n \times r^{-n} \times$ co-efficient of the term; and the errors of every subsequent term x^{h+n}, will be nearly as $\pm m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3} \cdot \frac{m-3}{4} \ldots \frac{m-h+1}{h} \times p^n \times r \times$ co-efficient of the term x^{h+n}, if for r, $r + p$, $r + 2p$, &c. be substituted $1, 1 + \frac{p}{r}, 1 + \frac{2p}{r}$, &c.

7. Let the preceding equation $sn = ns(n-1) - n \cdot \frac{n-1}{2} s(n-2) + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot s(n-3) -$ &c. $= n \times \log.(r - p) - n \cdot \frac{n-1}{2} \log.(r - 2p) + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \log.(r - 3p) +$ &c. but $\log. r - n \times \log.(r - p) + n \cdot \frac{n-1}{2} \log.(r - 2p) - n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \times \log.(r - 3p) +$ &c. $= \log.$

$\frac{r \times (r-2p)^{s} \times (r-4p^{s'}) \times (r-6p^{s''}) \times \&c.}{(r-p^{t}) - (r-3p^{t'}) \times (r-5p^{t''}) \times \&c.} = \log.$ K, where s, s', s'', &c. denote the co-efficients of the alternate terms of the binomial theorem, viz. $s = n \cdot \frac{n-1}{2}$, $s' = n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4}$, &c., and $t = n$, $t' = n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3}$, &c. the co-efficients of the remaining alternate terms; the numerator $r \times (r-2p^{s}) \times (r-4p)^{s'} \times (r-6p)^{s''} \times$ &c. $=$ (if $N = 2^{n-1}$) $r^{N} - Ppr^{N-1} + Qp^{2}r^{N-2} - Rp^{3}r^{N-3} \ldots Lp^{n-1} \times r^{N-n+1} \pm Mp^{n}r^{N-n} \mp$ &c. and the denominator $(r-p)^{t} \times (r-3p)^{t'} \times (r-5p)^{t''} \times$ &c. $= r^{N} - Ppr^{N-1} + Qp^{2}r^{N-2} - Rp^{3}r^{N-3} + \ldots Lp^{n-1}r^{N-n+1} (+M \pm 1 \cdot 2 \cdot 3 \ldots n-1) p^{n}r^{N-n} \mp$ &c. whence the numerator and denominator have the n first terms the same, and the next succeeding terms differ by $1 \cdot 2 \cdot 3 \ldots (n-1) p^{n}r^{N-n}$; the numerator divided by the denominator $= \pm \frac{1 \cdot 2 \cdot 3 \ldots n-1}{r^{n}} p^{n}$ nearly, if r be a great number in proportion to p, &c. it would be $+$ when n is an odd number, and $-$ when even.

8. The logarithm of the fraction K by the common series $= K - 1 - \frac{(K-1)^{2}}{2} + \frac{(K-1)^{3}}{3} -$ &c. has for its first term $= \pm \frac{1 \cdot 2 \cdot 3 \ldots n-1}{r^{n}} \times p^{n}$ nearly; for its 2d term the square of the first divided by 2, &c.

9. The error of this equation not only depends on the logarithm of K, which may be calculated to any degree of exactness, but in the calculus on the errors of the given logarithms.

10. If r be increased or diminished by any given number, the n first terms of the numerator and denominator will still result the same, and the next succeeding terms will differ by $1 \cdot 2 \cdot 3 \cdot 4 \ldots n-1 \times p^{n} \times r^{N-n}$.

11. Let $n \cdot \frac{n-1}{2}$ numbers be 2, $n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4}$ numbers be 4, $n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} \cdot \frac{n-4}{5} \cdot \frac{n-5}{6}$ numbers be 6, &c.; their sum, the sum of the products of every 2, the contents of every 3, 4, 5, &c. to $n-1$ of them, will be equal to the sum, the sum of the products of every 2, of the contents of every 3, 4, 5, &c. to $n-1$ of the following numbers, viz. n numbers which are 1, $n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3}$ numbers which are 3, $n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} \cdot \frac{n-4}{5}$ which are 5, &c.; and the sum of the contents of every n of the former will be less than the sum of the contents of every n latter numbers by $1 \cdot 2 \cdot 3 \cdot 4 \ldots n-1$.

12. The method given in art. 4, which I name a method of correspondent values, easily deduces and demonstrates the preceding equations, which cannot, without much difficulty, be done by the preceding method of differences; the method of correspondent values is much preferable to the method of differences, both for the facility of its deduction, and the generality of its resolution: for instance, from this method very easily can be deduced, &c. the subsequent and other similar equations.

1

Exam. 1. $sn = ns\,(n-1) - n\,.\,\frac{n-1}{2}\,s\,(n-2) + n\,.\,\frac{n-1}{2}\,.\,\frac{n-2}{3}\,s\,(n-3)$ ... $\pm\, ns1 \mp s$ nearly.

Exam. 2. $s\,(n+m) = \frac{m+n\,.\,m+n-1\,.\,m+n-2\ldots m+2}{1\,.\,3\,..\,n-1} \times s\,(n-1) - \frac{n-1}{1} \times A \times \frac{m+1}{m+2} \times s\,(n-2) + \frac{n-2}{2} \times B \times \frac{m+2}{m+3} s\,(n-3) - \frac{n-3}{3} \times C \times \frac{m+3}{m+4} \times s\,(n-4) + \frac{n-4}{4} \times D \times \frac{m+4}{m+5} \times s\,(n-5)$ — &c. nearly, where the letters A, B, C, D, &c. denote the preceding co-efficients, and the converging series is the same as in the preceding example.

Exam. 3. Let the converging series be of the formula $ax + bx^3 \times cx^5 + dx^7 +$ &c.; then will $sn = (2n-2)\,s\,(n-1) - (2n-1) \times \frac{2n-4}{2} s\,(n-2) + (2n-1) \times \frac{2n-2}{2} \times \frac{2n-6}{3} s\,(n-3) - (2n-1)\,.\,\frac{2n-2}{2}\,.\,\frac{2n-3}{2} \times \frac{2n-4}{4} s(n-4)$ + &c. nearly, of which the general term is $(2n-1)\,.\,\frac{2n-2}{2}\,.\,\frac{2n-3}{3}\,..\,\frac{2n-l+1}{l-1} \times \frac{2n-2l}{l} \times s\,(n-l)$.

These series may be made to begin from any term, which may be easily found by the method of correspondent values, and the subsequent terms from it by the given law; its preceding terms may be deduced from the same law reversed, that is, by putting the numerators of the fractions multiplied into it for the denominators, and the denominators for the numerators. From these different serieses may be formed, by adding 2 or more terms of the given series together for a term of the required series; which method has been applied to converging series in general in the Meditationes.

13. The method of correspondent values easily affords a resolution of the problems contained in Mr. Brigg's or Sir Isaac Newton's method of differences.

Exam. 1. Let the quantity be of the formula $a + bx + cx^2 + dx^3 +$ &c... $x^n = y$, and $n + 1$ correspondent values of x and y be given, viz. $p, q, r, s,$ &c. of x; $sp, sq, sr, ss,$ &c. of y; then will $y = \frac{x-q\,.\,x-r\,.\,x-s\,.\,\&c.}{p-q\,.\,p-r\,.\,p-s\,.\,\&c.} \times sp + \frac{x-p\,.\,x-r\,.\,x-s\,.\,\&c.}{q-p\,.\,q-r\,.\,q-s\,.\,\&c.} \times sq + \frac{x-p\,.\,x-q\,.\,x-s\,.\,\&c.}{r-p\,.\,r-q\,.\,r-s\,.\,\&c.} \times sr$ + &c. The truth of this problem very easily appears by writing $p, q, r, s,$ &c. for x in the given series.

All the preceding examples may be applied to this case, by writing x for m in the given series; hence the resolutions of several cases of equi-distant ordinates by easy and not inelegant serieses, among which are included the 2 cases commonly given on this subject.

14. If a quantity be required, which proceeds according to the dimensions of x; reduce the above given value of y into a quantity proceeding according to the dimensions of x, and there results $y =$

$$\left(\frac{sp}{p-q\,.\,p-r\,.\,p-s\,.\,\&c. = A} + \frac{sq}{q-p\,.\,q-r\,.\,q-s\,.\,\&c. = B}\right) \times x^n - \left(\frac{sp \times (q+r+s+\&c.)}{A}\right.$$

$$+ \frac{sq \times (p + r + s + \&c.)}{B} + \frac{sr \times (p + q + s + \&c.)}{C} + \&c.)\, x^{n-1} + (\frac{sp \times (qr + qs + rs + \&c.)}{A} + \frac{sq \times (pr + ps + rs + \&c.)}{B} + \frac{sr \times (pq + ps + qs + \&c.)}{C} + \&c.) \times x^{n-2} - (\frac{sp \times (qrs + \&c.)}{A} + \frac{sq \times (prs + \&c.)}{B} + \frac{sr \times (pqs + \&c.)}{C} + \&c.)\, x^{n-3} + \&c.$$

The law and continuation of this series is evident to any one versant in these matters from inspection. And these fractions may be reduced to a common denominator by substituting for sp and A the products $sp \times$ P and A $\times$ P, where P $= q - r \,.\, q - s \,.\, r - s \,.$ &c.; for sq and B the products $sq \times$ Q and B $\times$ Q, where Q $= p - r \,.\, p - s \,.\, r - s \,.$ &c.; for sr and C the products $sr \times$ R and C $\times$ R, where R $= p - q \,.\, p - s \,.\, q - s \,.$ &c.; for ss and D the products $ss \times$ S′ and C $\times$ S′, where S′ $= p - q \,.\, p - r \,.\, q - r \,.$ &c. &c.

The fractions, in particular cases, will often be reducible to lower terms.

15. Let $y = ax^b + bx^{b+l} + cx^{b+2l} +$ &c., and the correspondent values of x and y be given as before, then will $y =$

$$\frac{x^h \times x^l - q^l \times x^l - r^l \times x^l - s^l \times \&c.}{p^h \times p^l - q^l \times p^l - r^l \times p^l - s^l \times \&c.} \times sp + \frac{x^h \times x^l - p^l \times x^l - r^l \times x^l - s^l \times \&c.}{q^h \times q^l - p^l \times q^l - r^l \times q^l - s^l \times \&c.} \times sq + \&c.$$

This series may, in the same manner as the preceding, be reduced to terms proceeding according to the dimensions of x; and the serieses given in the examples may (mutatis mutandis) be predicated of it.

16. A more general method of correspondent values is given in the Meditationes, as also the subsequent $y = \frac{x - q . x - r . x - s . \&c.}{p - q . p - r . p - s . \&c.} \times sp + \frac{x - p . x - r . x - s . \&c.}{q - p . q - r . q - s . \&c.} \times sq$ + &c. as in exam. 1, $= sp + (x - p)\,(\frac{1}{p-q} \times sp + \frac{1}{q-p} \times sq) + (x - p)\,(x - q)\,(\frac{1}{p-q} \times \frac{1}{p-r} \times sp + \frac{1}{q-p} \times \frac{1}{q-r} \times sq + \frac{1}{r-p} \times \frac{1}{r-q} \times sr) + (x - p)\,(x - q)\,(x - r)\,(\frac{1}{p-q} \cdot \frac{1}{p-r} \cdot \frac{1}{p-s} \cdot \times sp + \frac{1}{q-p} \cdot \frac{1}{q-r} \cdot \frac{1}{q-s} \times sq + \frac{1}{r-p} \cdot \frac{1}{r-q} \cdot \frac{1}{r-s} \times sr + \frac{1}{s-p} \cdot \frac{1}{s-q} \cdot \frac{1}{s-r} \times ss) -$ &c.

The equality of these 2 different quantities will easily appear by finding the co-efficients of both, which are multiplied into the same given value of y as sp, sq, sr, &c. and the same power of x; for with very little difficulty they will in general be found equal.

It is evident from this resolution that, giving the ordinates and their respective distances from each other, the value of any other ordinate at a given distance from the preceding, found by this method, will result the same, whatever may be the point assumed from which the absciss is made to begin.

§ 3.—1. Let a series be $Ax + Bx^2 + Cx^3 + Dx^4 +$ &c. of such a formula, that if in it for x be substituted $a + b$, there results a series $A \times (a + b) + B \times (a + b)^2 + C \times (a + b)^3 + D \times (a + b)^4 +$ &c. $= (Aa + Ba^2 + Ca^3 + Da^4 + \&c.) \times (1 + qb + rb^2 + sb^3 + tb^4 + \&c.) + (1 + qa + ra^2 + sa^3 + ta^4 +$

&c.) $\times$ ($\mathrm{A}b + \mathrm{B}b^2 + \mathrm{C}b^3 + \mathrm{D}b^4 +$ &c.); then will the series $\mathrm{A}x + \mathrm{B}x^2 + \mathrm{C}x^3 + \mathrm{D}x^4 +$ &c. $= \mathrm{A}x + \frac{2\mathrm{B}}{1.2}x^2 + \frac{2.3\mathrm{C}}{1.2.3}x^3 + \frac{24\mathrm{ABC} - 8\mathrm{B}^3}{1.2.3.4\mathrm{A}^2}x^4 + \frac{36\mathrm{C}^2\mathrm{A}^2 + 24\mathrm{ACB}^2 - 16\mathrm{B}^4}{1.2.3.4.5\mathrm{A}^3}x^5 +$ &c.; and the series $1 + qx + rx^2 + sx^3 + tx^4 +$ &c $=$
$$1 + \frac{\mathrm{B}}{\mathrm{A}}x + \frac{6\mathrm{CA} - 2\mathrm{B}^2}{1.2\mathrm{A}^2}x^2 + \frac{18\mathrm{CAB} - 8\mathrm{B}^3}{1.2.3\mathrm{A}^3}x^3 + \frac{36\mathrm{C}^2\mathrm{A}^2 - 8\mathrm{B}^4}{1.2.3.4\mathrm{A}^4}x^4 + \text{\&c.}$$

The terms of these 2 serieses can easily be deduced by the subsequent method. Let $\mathrm{K}x^{n-2} + \mathrm{L}x^{n-1} + \mathrm{M}x^n$, be successive terms of the series $\mathrm{A}x + \mathrm{B}x^2 + \mathrm{C}x^3 +$ &c., and $\mathrm{K}'x^{n-2} + \mathrm{L}'x^{n-1}$ successive terms of the series $1 + qx + rx^2 + sx^3 + tx^4 +$ &c.; then will $\mathrm{M} = \frac{2\mathrm{A}^2 \times \mathrm{B} \times \mathrm{K}' + 6\mathrm{CAK} - 2\mathrm{B}^2\mathrm{K}}{n.(n-1) \times \mathrm{A}^2}$, and $\mathrm{L}' = \frac{n \times \mathrm{A} \times \mathrm{M} - \mathrm{B} \times x\mathrm{L}}{\mathrm{A}^2}$.

Cor. 1. Let $\mathrm{B} = 0$, and the 2 serieses $\mathrm{A}x + \mathrm{B}x^2 + \mathrm{C}x^3 + \mathrm{D}x^4 +$ &c. and $1 + qx + rx^2 +$ &c. become respectively,
$\mathrm{A}x + \frac{2.3}{2.3}\mathrm{C}x^3 + \frac{2^2.3^2}{2.3.4.5} \times \frac{\mathrm{C}^2}{\mathrm{A}}x^5 + \frac{2^3.3^3}{2.3.4.5.6.7} \times \frac{\mathrm{C}^3}{\mathrm{A}^2} \times x^7 +$ &c., and $1 + \frac{2.3}{1.2} \times \frac{\mathrm{C}}{\mathrm{A}}x^2 + \frac{2^2.3^2}{1.2.3.4} \times \frac{\mathrm{C}^2}{\mathrm{A}^2}x^4 + \frac{2^3.3^3}{1.2.3.4.5.6} \times \frac{\mathrm{C}^3}{\mathrm{A}^3}x^6 +$ &c.

If in these serieses for A be substituted 1, and for C be substituted $-\frac{1}{2.3}$, there will result the serieses $x - \frac{x^3}{2.3} + \frac{x^5}{2.3.4.5} -$ &c., and $1 - \frac{x^2}{1.2} + \frac{x^4}{1.2.3.4} -$ &c. which give the sine and cosine in terms of the arc x.

Cor. 2. Let $\mathrm{C} = 0$, and the above-mentioned series $\mathrm{A}x + \mathrm{B}x^2 +$ &c. becomes $\mathrm{A}x + \frac{2}{1.2}\mathrm{B}x^2 * - \frac{2^3}{1.2.3.4} \times \frac{\mathrm{B}^3}{\mathrm{A}^2}x^4 - \frac{2^4}{1.2.3.4.5} \times \frac{\mathrm{B}^4}{\mathrm{A}^3}x^5 +$ &c. The law of this series is, first, that every 3d term vanishes; and 2dly, the signs of every 2 successive terms change alternately from + to − and − to +; and lastly, the co-efficient of the term x^n is $\frac{2^{n-1}}{1.2.3..n} \times \frac{\mathrm{B}^{n-1}}{\mathrm{A}^{n-2}}$; and the series $1 + qx + rx^2 +$ &c. becomes $1 + \frac{\mathrm{B}}{\mathrm{A}}x - \frac{2\mathrm{B}^2}{1.2\mathrm{A}^2}x^2 - \frac{2^3\mathrm{B}^3}{1.2.3\mathrm{A}^3}x^3 - \frac{2^3\mathrm{B}^4}{1.2.3.4\mathrm{A}^4}x^4 +$ &c. In this series the signs of 3 successive terms alternately change from + to − and − to +; and the co-efficient of the term x^n is $\frac{2^n \times \mathrm{B}^n}{1.2.3.n\mathrm{A}^n}$ or $\frac{2^{n-1} \times \mathrm{B}^n}{1.2.3..n\mathrm{A}^n}$ according as n is divisible by 3 or not.

2. Let a series $1 + \mathrm{P}x + \mathrm{Q}x^2 + \mathrm{R}x^3 + \mathrm{S}x^4 + \mathrm{T}x^5 +$ &c. be of such a formula, that if in it for x be substituted $a + b$, there results a series $1 + \mathrm{P} \times (a + b) + \mathrm{Q} \times (a + b)^2 + \mathrm{R} \times (a + b)^3 + \mathrm{S} \times (a + b)^4 +$ &c. $= (1 + \mathrm{P}a + \mathrm{Q}a^2 + \mathrm{R}a^3 + \mathrm{S}a^4 +$ &c.) $\times$ ($1 + \mathrm{P}b + \mathrm{Q}b^2 + \mathrm{R}b^3 + \mathrm{S}b^4 +$ &c.) + ($\mathrm{A}a + \mathrm{B}a^2 + \mathrm{C}a^3 + \mathrm{D}a^4 +$ &c.) $\times$ ($\mathrm{A}b + \mathrm{B}b^2 + \mathrm{C}b^3 + \mathrm{D}b^4 +$ &c.), then will the series $\mathrm{A}x + \mathrm{B}x^2 + \mathrm{C}x^3 + \mathrm{D}x^4 +$ &c. $= \mathrm{A}x + \mathrm{B}x^2 + (\frac{2\mathrm{B}^2}{3\mathrm{A}} - \frac{\mathrm{PB}}{3} + \mathrm{A} \times \frac{\mathrm{A}^2 + \mathrm{P}^2}{6})x^3 +$ &c., and the series $1 + \mathrm{P}x + \mathrm{Q}x^2 + \mathrm{R}x^3 +$ &c. $= 1 + \mathrm{P}x +$
$$\frac{\mathrm{A}^2 + \mathrm{P}^2}{2}x^2 + \frac{2\mathrm{AB} + \mathrm{P} \times (\mathrm{A}^2 + \mathrm{P}^2)}{6}x^3 + \frac{4\mathrm{B}^2 + (\mathrm{A}^2 + \mathrm{P}^2)^2}{24}x^4 + \text{\&c.}$$
Let $\mathrm{K}x^{n-2} + \mathrm{L}x^{-} + \mathrm{M}x^n$ be successive terms of the series $\mathrm{A}x + \mathrm{B}x^2 + \mathrm{C}x^3 +$ &c.. and $\mathrm{K}'x^{n-2} + \mathrm{L}'x^{n-1} + \mathrm{M}'x^n$ successive terms of the series $1 + \mathrm{P}x + \mathrm{Q}x^2 + \mathrm{R}x^3 +$ &c.;

then will $A \times L + P \times L' = n \times M'$ and $B \times K + Q \times K' = n.\frac{n-1}{2} \times M'$ express the law of the serieses.

Cor. Let $B = 0$, then the series $Ax + Bx^2 + Cx^3 + Dx^4 = A \times (x + \frac{P^2 \times A^2}{2.3}x^3 + \frac{(P^2 + A^2)^2}{1.2.3.4.5}x^5 + \&c.)$, and the series $1 + Px + Qx^2 + Rx^3 + \&c. = 1 + Px + \frac{P^2 + A^2}{1.2}x^2 + P \times \frac{P^2 + A^2}{1.2.3}x^3 + \frac{(P^2 + A^2)^2}{1.2.3.4}x^4 + P \times \frac{(P^2 + A^2)^2}{1.2.3\ldots5}x^5 + \frac{(P^2 + A^2)^3}{1.2.3..6}x^6 + \&c.$; the co-efficient of the term x^n will be $(P^2 + A^2)^{\frac{n}{2}}$ or $P \times (P^2 + A^2)^{\frac{n-1}{2}}$, according as n is even or odd.

If in the equations before given for x be substituted $a = b$ instead of $a + b$, then in the other quantities for b substitute $- b$.

3. If in case 2 the difference between the two quantities $(1 + Pa + Qa^2 + \&c.) \times (1 + Pb + Qb^2 + \&c.)$ and $(Aa + Ba^2 + Ca^3 + \&c.) \times (Ab + Bb^2 + Cb^3 + \&c.)$ is assumed $= 1 + P \times (a + b) + Q \times (a + b) + \&c.$, then in the serieses before given for A, B, C, &c. write respectively $\surd - 1A$, $\surd - 1B$, $\surd - 1C$, &c., and there will result the corresponding serieses.

The same principles may be applied to many other cases.

4. Equations of these formulæ may be useful, when the sums of the serieses correspondent to a value (a) of x are given, and the sums of the series correspondent to a value $(a + b)$ of x is required, b having a small ratio to a: for instance, let the given series be $x - \frac{x^3}{2.3} + \frac{x^5}{2.3.4.5} - \frac{x^7}{2.3..7} + \&c.$; the equation found in the first case is $a + b - \frac{(a+b)^3}{2.3} + \frac{(a+b)^5}{2.3.4.5} - \&c. = (a - \frac{a^3}{2.3} + \frac{a^5}{2.3.4.5} - \&c.) \times (1 - \frac{b^2}{1.2} + \frac{b^4}{1.2.3.4} - \&c.) + (1 - \frac{a^2}{1.2} + \frac{a^4}{1.2.3.4} - \&c.) \times (b - \frac{b^3}{2.3} + \frac{b^5}{2.3.4.5} - \&c.)$; but $a - \frac{a^3}{2.3} + \frac{a^5}{2.3.4.5} - \&c.$, and $1 - \frac{a^2}{1.2} + \frac{a^4}{2.3.4} - \&c.$ are the sine s and cosine c of an arc a of a circle whose radius is 1; and consequently, if the sine s and consine c of an arc a be given, the sine of an arc $a + b = s \times (1 - \frac{b^2}{2} + \frac{b^4}{2.4} - \&c.) + c\,(b - \frac{b^3}{2.3} + \frac{b^5}{2.3.4.5} - \&c.)$, which series, if b be very small in proportion to a, converges much faster than the common series for finding the sine from the arc: it has been given from different principles in the Meditationes, and is also easily deducible from the series for finding the sine and cosine from the arc by the propositions usually given in plane trigonometry: the cosine of the same arc $a + b = c \times (1 - \frac{b^2}{1.2} + \frac{b^4}{2.3.4} - \&c.) - s \times (b - \frac{b^3}{1.2.3} + \frac{b^5}{1.2..5} - \&c.)$

5. Let a quantity P be a function of x, or the fluent of a function of $x \times \dot{x}$,

and the value X of it when $x = a$ be known, and the value of it when $x = a + b$ be required. Find a series of which the first term is X, and which proceeds according to the dimensions of b, if b be a very small quantity, and in general at least so small that the series from $x = a$ to $x = a + b$ neither becomes infinite nor O. In the same manner, if an algebraical or fluxional equation or equations, expressing the relations between x, y, z, v, &c. be given, find the correspondent values of y, z, v, &c. to $x = a$, which let be Y, Z, V, &c.; then find serieses for y, z, v, &c. of which the first terms let be Y, Z, V, &c. respectively, and which proceed according to the dimensions of b, but subject to the same conditions as in the preceding case. From fluxional equations may be deduced series which express the value of y, &c. in terms of x, and always diverge, or always converge, whatever may be its value, as appears from the Meditationes.

XVIII. On the Resolution of Attractive Powers. By Edw. Waring, M. D., F. R. S., &c. p. 185.

1. A force acting at a given point may be resolved by an infinite number of ways into 2, 3, or more (n) forces acting at the same point, either in the same or different planes with the given force and each other; and, vice versâ, any number of such forces acting in the same or different planes may be reduced into one.

Exam. fig. 5, pl. 6. Let a body A be acted on by 3 forces AB, AC, and AD, not being in the same plane; reduce any 2 of them AB and AC to one AE, by completing the parallelogram ABEC; then reduce the 2 forces AE and AD to one AF by completing the parallelogram AEFD; then the 3 forces AB, AC, and AD, are reduced to the one AF.

2. If n forces act on the body A at the same time, and any $(n - 1)$ of them be reduced to 1, the force resulting will be situated in the same plane with the remaining, and force equivalent to the (n) forces.

3. If one force a be resolved into several others x, y, z, v, &c. situated in different planes, and the sines of the angles, which the forces y, z, v, &c. contain with the plane made by the direction of the forces x and a be respectively s, s', s'', &c. then will $sy \pm s'z \pm s''v \pm$ &c. $= 0$.

PROB. 1. fig. 6.—Given the law of attraction of each of the parts of a given line in terms of their distance from a given point P; to find the attraction of the whole line ab on the point P.—Find the attraction of the line ab on the point P in the 2 directions Pf and fb by the following method. Draw Px from the point P to any point x of the line ab; then the force acting on the point P by the particle $\dot{x}y$ will be the given function (determined from the given law of attraction) of the distance into the particle; draw also Ph perpendicular from the point P to the line ab; and let P$f = a$, $hf = b$, and $fx = y$; then will the

distance $PX = \surd(a^2 \pm 2by + y^2)$, and the function of the distance into the particle $\dot{xy} = \varphi \surd(a^2 \pm 2by + y^2) \times \dot{y} = F\,(y) \times \dot{y}$; let this be denoted by lx situated in the line Px, which resolve into 2 others $nx = \frac{y\dot{y} \times F:(y)}{Px = \surd(a^2 \pm 2by + y^2)}$ situated in the line ab, and ln (in a direction parallel to Pf) $= \frac{a\dot{y} \times F:(y)}{\surd(a^2 \pm 2by + y^2)}$; find the fluents of the fluxions $\frac{y\dot{y} \times F:(y)}{Px}$ and $\frac{a\dot{y} \times F:(y)}{Px}$ contained between the values af and fb of the line $fx = y$, which suppose Y and V respectively; through the point p draw Py parallel to $fb = Y$, and in the line Pf assume $Pu = V$; complete the parallelogram $Puzy$; Pz will be the force of the line ab on the point P.

Cor. If $F:(y)$ varies as any power or root $(2n)$ of the distance $Px = \surd a^2 \pm 2by + y^2)$, and $n - \frac{1}{2}$ be an integer affirmative number or 0, the fluents Y and V of both the fluxions can be found in finite algebraical terms of y; if $n - \frac{1}{2}$ be an integer negative number, both the fluents can be found in the above mentioned finite terms, together with the arc of a circle whose radius is $\surd \overline{a^2 - b^2}$ and tangent $y \mp b$, unless $n - \frac{1}{2} = -1$, in which case the fluent Y involves that circular arc, and also the logarithm of $y^2 \pm 2by + a^2$. If $n - \frac{1}{2}$ denotes a fraction whose denominator is 2, both the fluents can be expressed by the finite terms together with the log. of $y \pm b + \surd(y^2 \pm 2by + a^2)$. If the fluents be given, when n is a given quantity, and $n - \frac{1}{2}$ not a whole affirmative number, from them can be deduced the fluents of any fluxions resulting by increasing or diminishing n by a whole number, unless in the above-mentioned case of $n - \frac{1}{2} = -1$. If $b = 0$, and consequently the line Pf is perpendicular to the given line ab, the fluent Y will be expressed by the finite terms, unless $n - \frac{1}{2} = -1$, in which case it will be as $\frac{1}{2}$ log. $(y^2 + a^2)$ when properly corrected. These fluxions $\dot{Y}$ and $\dot{V}$ may be transformed into others, whose variable quantity is $Px = u$ the distance from P, by substituting in the fluxions for y and $\dot{y}$ their respective values $\surd(u^2 - a^2 + b^2) \mp b$ and $\frac{u\dot{u}}{\surd(u^2 - a^2 + b^2)}$, and consequently for $\surd(y^2 \pm 2by + a^2)$ its value u.

PROB. 2, fig. 7. Given the attraction of each of the parts of a given surface in terms of their distance from a given point P, and an equation expressing the relation between an absciss $Ap = x$, and its correspondent ordinates $pm = y$ of the surface; to find the attraction of the surface on the given point P.

First, by the preceding proposition find the attractions Y and V of any ordinate $m\,p\,m'$ in the directions of the ordinate pm and of the line Pp; and from the equation expressing the relation between the absciss and ordinates of the given curve, find the absciss in terms of the ordinates $(pm) = \pi:(y)$, and thence $\dot{x} = \varphi:(y) \times \dot{y}$ and $\surd(a'^2 \pm 2sa'x + x^2) = \varphi':(y)$, where $PA = a'$, and $s =$ cosine of the angle which the absciss Ap makes with the line PA; then find the fluents of the 3 fluxions $\dot{x} \times Y = \dot{y} \times Y \times \varphi:(y)$, $\dot{x} \times \frac{V \times x}{\surd a'^2 \pm 2sa'x + x^2} = \varphi:$

$(y) \times \dot{y} \times \frac{\pi : (y)}{\varphi : (y)} \times$ V and $\dot{x} \times \frac{a'v}{\sqrt{(a'^2 \pm 2sa' x + x^2)}} = \dot{y} \times \frac{a'v}{\varphi' : (y)}$, contained between the values of y, which correspond to the extreme values of x, which suppose Y′, V′, and Z; and draw through the point P the lines Py and PZ respectively parallel to the ordinates pm and to the absciss Ap, and equal to $r \times$ Y′ and V′; assume Pu in the line (PA) $= t \times$ Z, r and t denoting the sines of the angles, which the ordinates pm and line AP make with the absciss Ap: reduce these 3 forces Py, PZ, and Pu, to one Pf; thence Pf will be the force of the surface on the point P.

Cor. 1. If for y and $\dot{y}$ be substituted their values in terms of x and $\dot{x}$, deduced from the equation expressing the relation between the absciss Ap and ordinate pm of the given curve, thence will be deduced the above-mentioned fluents Y, V, Y′, V′, and Z, in terms of x: and in the same manner, if for x and $\dot{x}$ be substituted in the fluxions or fluents resulting their values $\sqrt{(u^2 - a'^2 + 1^2 a'^2)} \mp sa'$, and its fluxion, there will result the above-mentioned fluxions or fluents in terms of u the distance from the point P.

Cor. 2. Let the curve be a circle, of which A is the centre, PA a line perpendicular to the plane of the circle, and the ordinate pm perpendicular to the absciss Ap; the forces on each side of the absciss Ap will be equal, and the force in the direction of the absciss Ap will be equal to that in the contrary direction; the force in the direction (PA) $= 4 \times \int \frac{a\dot{u}}{\sqrt{(u^2 - a^2)}} \times \int \frac{u\dot{y}}{\sqrt{u^2 + y^2}} \times$ F$: \sqrt{(u^2 + y^2)}$ = W, in which F $: \sqrt{u^2 + y^2}$ is the function of the distance, according to which the given force on the particles varies; the fluent $\int \frac{u\dot{y}}{\sqrt{(u^2 + y^2)}} \times$ F $: \sqrt{(u^2 + y^2)}$ is contained between the values 0 and $\sqrt{(r^2 + a^2 - u^2)}$ of the quantity y, and the fluent W is contained between the values a and $\sqrt{(a^2 + r^2)}$ of the quantity u, where $a =$ PA and r the radius of the circle; but the same force is $= 2 \times$ 3.14159 &c. $\times \int a\dot{u} \times$ F $: u$, where F $: u$ denotes the given function of the distance u, and the fluent is contained between the values a and $\sqrt{a^2 + r^2}$ of u.

PROB. 3. To find the attraction of a given solid on a given point P. Find the attraction of every parallel section on that point by the preceding problem, and multiply it into the correspondent fluxion of the first abscissa AP; and also find the fluent of the resulting fluxion, which, properly corrected, multiply into the sine of the angle which the first abscissa makes with the parallel sections, and the product will be proportional to the attraction of the solid on the given point P.

2. Fig. 8. Let the solid ABCH be generated by the rotation of a given curve round its axis AB, which passes through the point attracted P, and this solid be supposed to consist of small evanescent solids, whose bases are the surfaces EF, ef, &c. of spheres of which the centre is P, and altitudes Ff, &c. the increments of the base AB contained between the 2 contiguous surfaces EF and ef: from the

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points E and e of the curve draw ED and ed perpendicular to the axis AB, and ES perpendicular to the arc Ee of the given curve at the point E, and meeting the axis AB in S: then will the evanescent solid EFfe $= p \times$ PE $\times$ FD $\times$ Ff $= p \times$ FD $\times$ PS $\times$ Dd (because Ff $= \frac{\text{PS} \times \text{D}d}{\text{PE}}$) $= p \times (\sqrt{(z^2 + y^2)} - z) \times (z\dot{z} \mp y\dot{y})$, where z and y denote respectively the absciss PD, and its correspondent ordinate DE of the given curve.

The increment of the attraction of the surface EF on the point P, in the direction PD, will be as the increment of the surface ($p \times$ PE $\times$ Dd) $\times \frac{\text{PD}}{\text{PE}} \times$ force of each particle $= p \times$ PD $\times$ Dd $\times$ given force of the particle; but the fluent of the fluxion PD $\times$ Dd contained between the points E and F is $= \frac{1}{2}\text{PE}^2 - \frac{1}{2}\text{PD}^2 = \frac{1}{2}\text{ED}^2$; whence the attraction of the evanescent solid EFfe is as $\frac{1}{2}p \times \text{ED}^2 \times$ Ff $\times$ F : $\sqrt{x^2 + y^2}$, force of each given particle at the distance (PE $= \sqrt{(x^2+y^2)}$) $= \frac{1}{2}p \times \text{ED}^2 \times \frac{\text{PS}}{\text{PE}} \times$ Dd $\times$ F : $\sqrt{z^2 + y^2} = \frac{1}{2}py^2 \times \frac{z\dot{z} \pm y\dot{y}}{\sqrt{(z^2 + y^2)}} \times$ F : $\sqrt{(z^2 + y^2)}$; the fluent of which, properly corrected, is as the attraction of the solid on the point P; p denoting the circumference of a circle whose radius is 1.

Cor. 1. The fluxion of this solid is $\frac{1}{2}py^2\dot{z} = \dot{\text{T}}$, which deduced from the preceding principles $= p \times (\sqrt{(z^2 + y^2)} - z) \times (z\dot{z} \mp y\dot{y}) = \dot{\text{V}}$, and consequently their fluents between two values of z, which correspond to two values of y=0, will be equal to each other.

Cor. 2. The increment of the attraction of this solid, as given in this proposition, $\frac{1}{2}p \times y^2 \times \frac{z\dot{z} \mp y\dot{y}}{\sqrt{(z^2 + y^2)}} \times$ F : $\sqrt{(z^2 + y^2} = \dot{v}$; but in the preceding proposition the force of a circle on the point P $= p \times \int a\dot{u} \times$ F : u, where $u = \sqrt{(z^2 + y^2)}$, and a = z, and y or u the only variable quantity contained in the fluxion; consequently the fluxion of the attraction of the solid $p \times \dot{z}$ $\int z \frac{y\dot{y}}{\sqrt{z^2 + y^2}} \times$ F : $(z^2 + y^2)^{\frac{1}{2}} = \dot{w}$; therefore, if for the fluent of $\frac{zy\dot{y}}{\sqrt{(z^2 + y^2)}} \times$ F : $(z^2 + y^2)^{\frac{1}{2}}$ be substituted its fluent contained between the values a and the value of y, which in the given equation corresponds to z; then the fluents of $\dot{v}$ and $\dot{w}$, contained between the 2 values of z which corresponds to 2 values of $y = 0$, will be equal to each other.

The difference of the fluents of $\dot{\text{T}}$ and $\dot{\text{V}}$, &c. contained between any other 2 values of z, can easily be deduced from the difference of 2 segments of spheres.

1. It may not be improper to remark in this place, that from different methods of finding the sum of quantities, the fluents of fluxions, the integrals of increments, &c. quantities may often be deduced equal, which otherwise cannot without some difficulty; of which instances are contained in the Meditationes, and I

shall here subjoin one more to those already given in this paper.—viz. *Exam.* Any curvilinear area ABC, &c. may be supposed to consist of evanescent areas EFef, of which the base EF is the arc of a circle, whose radius is PE $= \sqrt{(z^2 + y^2)}$ and sine ED $= y$, and altitude Ff, and consequently the fluxion of the area $=$ Ff $\times$ arc A of a circle whose radius is PE and sine ED $= \frac{\text{PS}}{\text{PE}} \times \dot{z} \times \text{A} = \frac{z\dot{z} \mp y\dot{y}}{\sqrt{(z^2 + y^2)}} \times \text{A} = \dot{v}$; the fluent of $\dot{v}$ contained between the 2 values of z which correspond to 2 values of $y = 0$, will be equal to the fluent of $y\dot{z}$ contained between the same 2 values of z.

2. From a similar method may be deduced equalities between other like fluents; for the curve may be supposed to consist of other similar curve surfaces equally as circles, and the solid of similar segments of other solids equally as spheres.

3. From the same principles may innumerable serieses equal to each other be deduced; for by different converging serieses find the sum of the same quantity or quantities, and there will result serieses equal to each other: for instance (fig. 9), if the time of falling down the arcs AC and BC, and their interpolations from the principles delivered in the Meditationes Analyticæ, of which the difference let be D; find the difference between the times of a body's falling through BC when it began to fall from A and from B by a series proceeding according to the dimensions of AB $= 0'$ a small quantity; and find, by a series of the same kind, the time of falling through AB; the sum of these 2 serieses will be equal to D. Similar propositions may be deduced from fluxional equations.

4. In some cases the ratios of the times of bodies falling through some particular distances to each other may be easily known; for instance, let the force vary as the $m - 1$ power of the distance x, and a be the distance from which the body began to fall; then the velocity varies as $\sqrt{(a^m - x^m)}$, and the increment of the time as $\frac{\dot{x}}{\sqrt{(a^m - x^m)}}$; but if the parts of different curves are proportional, then will a, x, and $\dot{x}$ vary in the same ratio as each other, and consequently the time through proportional parts of the distance will vary as $a^{1-\frac{1}{2}m}$; and if the bodies be resisted likewise by a force which varies as the $\frac{2m-2}{m}$ power of the velocities, then the times through proportional parts will vary as before, that is, as $a^{1-\frac{1}{2}m}$, where a denotes the proportional distances from the points where the forces and resistances are equal.

PROB. 4.—1. Fig. 10. Given an equation expressing the relation between the 2 abscissæ z $=$ AP and $x =$ Pp, and their correspondent ordinates $y = pm$ of a solid; to find its solid contents contained between 2 values of its first abscissæ z. Assume z as an invariable quantity, and from the equation resulting find the

fluent z of $y\dot{x}$ contained between the extreme values of x or y; then find the fluent of $z\dot{z}$ contained between the given values of z; then the fluent multiplied into the product of the sines of the angles which the first abscissa makes with the plane of the ordinates and 2d absciss, and the 2d absciss makes with its correspondent ordinates, will be the solid content required.

2. Fig. 11. Let the 1st absciss z of a solid be perpendicular to the planes of the ordinates, and the 2d absciss $Pp = x$ perpendicular to the ordinates themselves $pm = y$. First, assume the 1st absciss as invariable, and find the increment of the arc $p'm = (\dot{x}^2 + \dot{y}^2)^{\frac{1}{2}}$, then assume the 2d absciss Pp as constant, and let mu be the fluxion of the ordinate y or u, when the fluxion of the first absciss is $\dot{z} = ul$, where ul is perpendicular to the plane of the ordinates $p'pm$, and l a point of the surface of the solid; draw uh perpendicular to the arc $p'm$: then since ul is constituted at right angles to the plane $pp'm$, lh will cut the arc $p'm$ at right angles; but $uh = \frac{um \times pp'}{p'm} = \frac{\dot{u}\dot{x}}{\sqrt{(\dot{x}^2 + \dot{y}^2)}}$; $lh = (hu^2 + lu^2)^{\frac{1}{2}} = (\frac{\dot{u}\dot{x}}{\dot{x}^2 + \dot{y}^2} + \dot{z}^2)^{\frac{1}{2}}$; the fluxion of the surface will be $lh \times \sqrt{(\dot{x}^2 + \dot{y}^2)}$. From the given equation expressing the relation between the 2 abscissæ z and x and ordinates y, find, by assuming z invariable, $p\dot{x} = \dot{y}$, and by assuming x invariable $q\dot{z} = \dot{y}' = \dot{u}$; which being substituted for their values in the quantity $lh \times \sqrt{(\dot{x}^2 + \dot{y}^2)}$, there will result $(q^2 + p^2 + 1)^{\frac{1}{2}} \times \dot{x} \times \dot{z} = \text{A}\dot{x}\dot{z} = \frac{(q^2 + p^2 + 1)^{\frac{1}{2}}}{p} \times \dot{y} \times \dot{z} = \text{B}\dot{y}\dot{z}$; in A and B for y and x respectively substitute their value deduced from the given equation, and let the resulting quantities be $\text{A}'\dot{x}\dot{z}$ and $\text{B}'\dot{y}\dot{z}$, where A′ is a function of x and z, and B′ a function of y and z; find the fluent of $\text{A}'\dot{x}\dot{z}$, from the supposition that x is only variable, contained between the extreme values of x to a given value of z, which let be $\text{L}\dot{z}$; then find the fluent of $\text{L}\dot{z}$ by supposing z only variable contained between given values of z; and it will be the surface of the solid contained between those values.

The same may be deduced by finding the fluent of $\text{B}'\dot{y}\dot{z}$ on the supposition that y is the only variable quantity contained between the extreme values of y, as before of x to a given value of z, which let be $\text{L}'\dot{z}$; then will the fluent of $\text{L}'\dot{z}$, contained between the given values of z, be the surface required. If the solid be a cone generated by the rotation of a rectangular triangle round a side containing the right angle as an axis; hu will be a given quantity, if $\dot{z}$ be given.—If the above-mentioned angles are given, but not right ones, the arc $p'm$ and perpendicular $l'h$ can easily be deduced, and consequently the increment of the surface.

3. To define a curve of double curvature, it is necessary to have 2 equations, expressing the relation between the abcissæ z and x and their ordinates y, given; and if the angles which they respectively make with each other be right ones, the fluxion of the arc, as given in the Proprietates Curvarum, is $(\dot{z}^2 + \dot{x}^2 + \dot{y}^2)^{\frac{1}{2}}$.

Find its value from the 2 given equations, in terms of x, y, or z, multiplied into its respective fluxions; then its fluent, properly corrected, will be the length of the arc required. If the angles are not right, they may easily be reduced to them.

4. The attractions of these surfaces, curves, &c. on a given point P, may be deduced from the preceding principles of finding the attractions of each of the parts in the directions of the first abscissa, which passes through the point P, the 2d abscissa, and the ordinates, and then finding the integrals of these increments. From the method which determines the attraction of a body, surface, &c. on a given point, can be determined the attraction of a body, &c. on any number of points, and consequently the attraction of one body, &c. on another, &c. It is sometimes advantageous to transform the first absciss, that it may pass through the point attracted; and the abscissæ and ordinates, that they may be at right angles to each other, &c.

PROB. 5.—1. Fig. 12. Given an equation expressing the relation between the 2 abscissæ AP and Pp of a solid, and their correspondent ordinates pm, or AP′, P′p', and $p'm'$; to transform the first abscissa into any other Lh.

Let the abscissa Lh begin from a point L of the first abscissa AP, and meet an ordinate pm in the point h; draw hp, and let the sines of the angles Ppm, Php, and pPh; LPh, PhL, and PLh, be denoted respectively by r, s, and t, and r', s', and t'; through a point h of the line Ph draw $p'h'm'$ parallel to pm, and make L$h = z$, $hh' = x$, and $h'm' = y$: in the given equation for AP, Pp, and pm, substitute respectively their correspondent values $\frac{s'z}{r'} \pm$ AL (a), $\frac{st'z}{rr'} \pm \frac{sx}{r}$ (for P$h = \frac{t'z}{r'}$ and Ph' = P$h \pm hh' = \frac{t'z}{r'} \pm x$), and $y \pm \frac{tt'z}{rr'} \pm \frac{tx}{r'}$; then there results an equation to the same solid, expressing the relation between the 2 abscissæ z = Lh and x, and their correspondent ordinates y.

1. 2. If the absciss Lh does not begin from L, a point in the first given absciss AP, but from M a point given out of it, it may be reduced to the preceding case, by drawing from M a line MN $= c$ to the plane of the 1st and 2d abscissæ parallel to the ordinates pm; and from N to the 1st abscissa a line NO $= b$ parallel to the 2d abscissæ, and substituting in the equation expressing the relation between AP, Pp, and pm for AP, Pp, and pm respectively $z \pm$ AO (a), $x \pm b$ and $y \pm c$; and there results the equation required expressing the relation between the 2 abscissæ z and x, and their correspondent ordinates y, of which the 1st abscissa z passes through the point M.

2. To change the 2d abscissa Pp into any other Lh, the 1st abscissa and ordinates remaining the same. In the preceding figure let L be considered as a moveable point of the first absciss AL, and the sines of the respective angles denoted by the same letters as before, and let L$h = x$, AL $= z$, and $hm = y$; in

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the given equation for AP, Pp, and pm, substitute $z \pm \frac{s'x}{r'}$, $\frac{st'x}{rr'}$, and $y \pm \frac{tt'x}{rr'}$; and there will result the equation required, expressing the relation between z and x the abscissæ, and their correspondent ordinates y.

3. Fig. 13. To change the ordinates, the abscissæ remaining the same, draw $p'm$ an ordinate transformed, $p'h$ parallel to the first abscissa AP, and meeting a 2d abscissa, of which pm is an ordinate in h: for the sines of the angles $p'hp$, hpp', and $hp'p$; $p'pm$, pmp', and $pp'm$, write r, s, and t, r', s', and t'; and for AP$'$, P$'p'$, and $p'm$, respectively z, x, and y; then substitute in the given equation for AP, Pp, and pm, their respective values z (AP$'$) $\pm \frac{st'}{rr'} \times y$, x (P$'p \pm \frac{ts'}{rr'} y$, and $\frac{t'}{r'}y$; and there results an equation to the solid expressing the relation between the 2 abscissæ AP$'$ and P$'p'$ and the transformed ordinates $p'm$.

From these cases, which are easily reducible to one, may be transformed any given abscissæ and their correspondent ordinates into any other containing given angles, &c. with the before-mentioned abscissæ and ordinates. In the properties of curve lines, first published in 1762, is given a method of deducing the equation to any section of the solid, and in particular the case of deducing the equation to the projection of any curve on a given plane. From the principles given in this, and the paper on centripetal forces, which the R. S. did me the honour to print, can be deduced the fluxional equations, whose fluents express the relations between the abscissæ and their correspondent ordinates, of the curves described by bodies of which the particles act on each other with forces varying according to given functions of their distances.

XIX. Experiments on the Congelation of Quicksilver in England. With further Experiments on the Production of Artificial Cold. By Mr. Richard Walker. p. 199.

Exper. 1.—On December 18, 1788, a favourable opportunity offered of beginning some experiments on the congelation of mercury. For this purpose Mr. W. prepared a mixture of diluted vitriolic acid (reduced by water till its specific gravity was to that of water as 1.5596 to 1) and strong fuming nitrous acid, of each equal parts. The glass tube of a mercurial thermometer, with its bulb half filled with mercury, was provided, as a convenient method of ascertaining when the mercury was congealed; for if, after being subjected to the cold of a frigorific mixture, the thermometer glass should be taken out and inverted, and the mercury found to remain completely suspended in that half of the bulb now uppermost, no doubt can remain of the success of the experiment; an hydrometer, with its lower bulb half an inch in diameter, and $\frac{3}{4}$ full of mercury, was also provided, in case any accident should happen to the other.

It may be proper to premise here, that in all experiments of this kind Mr. W. removes each vessel, when the liquor it contains is sufficiently cooled, out of the mixture in which it is immersed for that purpose, immediately previous to adding the snow or salts with intention to generate a still further increase of cold; and also prefers adding the snow or powdered salts to the liquor, instead of pouring the liquor on these: it is necessary also to stir about the snow or salts, while cooling in a frigorific mixture, from time to time, otherwise it will freeze into a hard mass, and frustrate the experiment.

A half-pint glass t mbler, containing $2\frac{1}{4}$ oz. of the above-mentioned diluted mixture of acids, being immersed in mixtures of nitrous acid and snow, till the liquor it contained was cooled to -30°, was removed out of the mixture and placed on a table; snow, likewise previously cooled in a frigorific mixture to -15°, was added by degrees to the liquor in the tumbler, and the mixture kept stirring till a mercurial thermometer sunk to -60°, where it remained stationary; the hydrometer was then immersed in the mixture (the thermometer glass having been broken in the course of the experiment,) and stirred about in it for a short time, and on taking the hydrometer out, and gently shaking it, the mercury had already acquired the consistence of an amalgam, and after immersing it again for a few minutes, and then taking out and inverting it, he was gratified for the first time with the sight of mercury in a state of perfect congelation. Mr. W. applied his hand to the inverted glass bulb; this soon loosened the solid mercury, which, on shaking the hydrometer, was distinctly heard to knock with force against the glass; it was then immersed a 2d time, and when taken out was found adhering to the glass as before. He now inverted the glass again, and kept it in that situation till the whole of the mercury melted, and dropped down globule after globule into the stem of the hydrometer. The interval of time from taking the mercury out of the frigorific mixture in a solid state, the last time, to its perfect liquefaction, was not noticed; but, on recollection immediately afterwards, was supposed to be not less than 3 or 4 minutes. In a succeeding experiment this circumstance was attended to, and the frozen mercury, weighing 7 scruples, was not entirely melted under 7 minutes, the temperature of the air $+30^{\circ}$.

The experiment which follows Mr. W. considers the most extraordinary, because it proves beyond a doubt, that mercury may be frozen not only here in summer, but even in the hottest climate, at any season of the year, by a combination of frigorific mixtures, in the way described in the Philos. Trans. vol. 77, p. 285, in which attempt to freeze mercury, made April 20, 1787, the temperature of the air and materials being $+45^{\circ}$, he certainly reached, without the assistance of snow or ice, the point of mercurial congelation; but had then no satisfactory proof that any part of the mercury was absolutely congealed.

Exper. 2. On December 30, 3 oz. of a mixture composed of strong fuming nitrous acid 2 parts, and strong vitriolic acid and water each 1 part, were cooled in a half-pint tumbler immersed in a frigorific mixture, till the temperature of the diluted mixture of acids was reduced to — 30°. The tumbler was then removed out of the mixture, and vitriolated natron (Glauber's salt) in very fine powder, previously cooled to — 14° by a frigorific mixture, added by degrees to the liquor in the tumbler, stirring it together till the mercury in the thermometer sunk to — 54°. The hydrometer used in the former experiment, with its lower bulb ¾ full of mercury, was now immersed and stirred about in the mixture for a few minutes, when on taking it out, and inverting it, he had the satisfaction to find the same proof of the mercury being frozen as in the former instance. Nearly 4 oz. of the powdered salt was added; but some was added after the greatest effect was produced. The temperature of the room in which these experiments were made was + 30° each time, and the mercury taken from a jar containing several pounds.

Exper. 3. By an experiment made purposely on January 10, 1789, Mr. W. found that mercury may be congealed tolerably hard, by adding fresh fallen snow, at the temperature of + 32°, to strong fuming nitrous acid, previously cooled to between — 25° and — 30°, which may be very easily and quickly effected by immersing the vessel containing the acid in a mixture of snow and nitrous acid. He used the fuming nitrous acid on all occasions, because that does not require to be diluted, cold being immediately produced on the smallest addition of snow.

Exper. 4. On January 12, at Dr. Thomson's request, Mr. W. repeated the experiment of freezing mercury, at the Anatomy School in Christ Church, in the presence of the Honourable Mr. Wenman, the Rev. Dr. Hoare, Dr. Sibthorp, junior, Dr. Thompson, the Rev. Mr. Jackson, of Christ Church, and Mr. Wood of this place, a gentleman well known for his ingenuity in mechanics. For this purpose were provided a spirit thermometer graduated very low, and a mercurial thermometer graduated to — 76°, two thermometer-glasses, with bulbs very near, if not quite an inch in diameter each, one filled with mercury nearly to the orifice of the tube, which was left open, the other with its bulb half filled, and an hydrometer with its lower bulb, considerably less than either of the others, likewise half filled with mercury; the temperature of the room at this time + 28°.

A pan, containing 9 oz. of the mixture of acids prepared as in the first experiment, was placed in a larger pan, containing nitrous acid, and this, in a frigorific mixture of nitrous acid and snow, contained in another pan much larger. When the nitrous acid in the 2d pan was cooled by this mixture to — 18°, and the mixed acids in the smallest pan nearly as much, snow at some-

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what between + 20° and + 25°, the temperature of the open air at that time, was added to the nitrous acid in the 2d pan, till the spirit thermometer sunk to near − 43°; then the thermometer, with its bulb half filled, was immersed a sufficient time, and when taken out, the mercury in it was found congealed, and adhering to the glass. The pan containing the mixed acids, and which had been removed while the snow was added to make the 2d mixture, was now replaced in it, in order to be cooled; and when the mixture of acids was reduced to the temperature of − 34°, snow previously cooled to − 18° was added, keeping the mixture stirred till the mercurial thermometer sunk to − 60°; its temperature by the spirit thermometer was then found to be − 51°.

The 3 glasses, containing the mercury to be frozen, were now immersed in this mixture, and having been moved about in it for a considerable time, during which the spirit thermometer rose scarcely 1°, were then severally taken out and examined. When the freezing mixture was supposed to have produced its effect, the bulb which was completely filled was taken out, and broken on a flat stone by a moderate stroke or 2 with an iron hammer. This bulb was 11 or 12 lines in diameter. The solid mercury was separated into several sharp and brilliant fragments, some of which bore handling for a short time before they returned to a fluid form. One mass, larger than the rest, consisting of nearly $\frac{1}{4}$ of the whole ball, afforded the beautiful appearance of flat plates, converging towards a centre. Each of these plates was about a line in breadth at the external surface of the ball, becoming narrower as it shot inwards. These facets lay in very different planes, as is common in the fracture of any crystallized ball, whether of a brittle metal or of the earths, as in balls of calcareous stalactite. The solid brittle mercury in the present instance bore a very exact resemblance, both in colour and plated structure, to sulphurated antimony, and especially to the radiated specimens from Auvergne, before they are at all tarnished.

Instead of a solid centre to this ball, it seemed as if there had been a central cavity, of about 2 lines in diameter, a considerable portion of which was evident in the fragment just described, at that part to which the radii converged. It is indeed possible, that this may have been merely the receptacle of some part of the mercury remaining fluid at the centre. The hollow within was shining, but its edges were neither soft nor mouldering; on the contrary, they were sharp and well defined: nor was the brilliancy of the radii attributable to any exudation of mercury as from an amalgam. In the 2 smaller bulbs, which were only half filled, the mercury preserved its usual lustre on the surface in contact with the glass, as well as on that surface which it had acquired in becoming solid. The latter was occupied by a conical depression, the gradations of which were marked by concentric lines. One of these hemispheres was struck with a hammer, as in the former instance, but was rather flattened and crushed than

broken. The other, on being divided with a sharp chissel, showed a metallic splendour on its cut surface, but not equalling the polish of a globule of fluid mercury. Thirteen ounces of snow in the whole were found to have been added to the mixed acids; but some was added to lower its temperature after the glasses containing the mercury were taken out, and the spirit thermometer had risen a few degrees. This was a day remarkably favourable for such an experiment. The thermometer exposed to the open air stood, at ½ past 8 this morning, at + 6°, which is a very extraordinary degree of cold here; but this experiment was not begun till noon.

Exp. 5. On Jan. 14, Mr. W. froze mercury at the Anatomy School again, in the presence of the Rev. the Dean of Christ Church, the Rev. Dr. Hornsby, and Dr. Thomson. Four ounces now of the mixture of acids, prepared as in the first experiment, were cooled in a tumbler to — 20°, which required somewhat more than an equal weight of snow, cooled nearly to the same temperature, to produce the greatest effect. This was somewhat less than in the last experiment, the spirit thermometer sinking no lower than — 46°, owing chiefly to the weather having become much warmer, the temperature of the open air being now + 36°. The mercurial thermometer immersed in this mixture sunk to — 55°, where it became stationary; then 2 thermometer glasses, one half filled with mercury, and the other filled to a considerable height up the tube, after being immersed some time, were examined. On breaking the shell of glass from the former of these, the mercury was found in a perfectly solid state; but its upper surface, which was highly polished, and of the colour of liquid mercury, instead of being only slightly depressed, as had been seen in every other instance which afforded an opportunity for inspection, now formed a perfectly inverted hollow cone. This great depression, as well as the concentric circles mentioned in a former instance, might be owing to a rotatory motion accidentally given to it while congealing. The solid mercury was beaten out; but having been suffered to lie some time on the table for inspection, very quickly melted into liquid globules. The flexibility of solid mercury was clearly to be observed in this beautiful specimen; for the external surface, particularly the upper thin rim of the concave part, was evidently bent by the first gentle stroke of the hammer. The globe of mercury in the other glass, which was very small, exhibited nearly the same phenomena, as in the instances before-mentioned.

It happened in these experiments, contrary to what has generally occurred to others, that the mercury never sunk lower than — 60°, seldom so low, in the thermometer, and but little below the point of mercurial congelation in the tubes of the thermometer glasses filled nearly up to the orifice, with a view to show the contraction of mercury in becoming solid by its great descent in the

tube. On reflecting on this circumstance afterwards, it occurred to Mr. W. that the further descent of the mercury in these experiments was prevented not solely by the mercury freezing in the tube, the cause commonly assigned, but rather by the quick formation of a spherical shell of solid mercury within the bulb, by the sudden generation of cold.

Dr. Beddoes expressing a desire to exhibit solid mercury at his lecture before his class, Mr. W. undertook to freeze some at the Laboratory on March 12th, and now resolved to satisfy himself respecting the cause which prevented the lower descent of the mercury in his former experiments. In this, as well as the former, the mercury in a thermometer graduated to — 60°, and likewise in a thermometer-glass, filled nearly to the orifice, which lengthened its scale to near — 250°, sunk only a few degrees below the point of mercurial congelation, and then remained stationary. After waiting some time, he took the thermometer out of the mixture, and observed the bulb apparently full, and the short thread of mercury above unbroken. He now embraced the lower part of the tube with his hand a few seconds, resting it on the upper part of the bulb; and on taking it away, he found that the whole of the mercury had subsided into the bulb, which it did not now quite fill, a small space at the top of the bulb remaining empty. He then took out the thermometer glass, and applied his hand to the tube; but the mercury remained stationary till he sunk his hand so as to communicate heat to that part of the bulb which is immediately connected with the tube, when the thread of mercury dropped entirely into the bulb. It was now immersed again for a short time, then taken out, and the shell of glass beaten off, which exposed a globe of solid mercury, nearly an inch in diameter. This bore several very smart strokes with a hammer before it began to liquefy, but was not perfectly malleable. In the course of these experiments, several fragments of the solid mercury were thrown into mercury in its ordinary liquid state, and were found to sink with considerable celerity.

In continuing his researches respecting the means of producing artificial cold, Mr. W. found that phosphorated natron produces rather more cold by solution in the diluted nitrous acid than the vitriolated natron. At the temperature of + 50°, 4 parts of the diluted nitrous acid, prepared by mixing strong nitrous acid with half its weight of water, required 8 parts of that neutral salt in fine powder to be added, in order to cause the thermometer to sink to — 6°; and again, by the addition of 5 parts of nitrated ammonia in fine powder, the thermometer sunk so low as — 16°, in the whole 66°. A mixture of this kind made the thermometer sink from 80°, the temperature of the materials before mixing, to 0°.

Mr. W. was directed to the trial of this salt, by the like remarkable sensation of coldness without pungency, which, with its other similar properties to ice,

first induced him, while pursuing the subject of cold, to try the effect of dissolving the vitriolated natron in the mineral acids. Equal quantities, by weight, of phosphorated natron and vitriolated natron, were evaporated separately over a gentle fire, till each was reduced to a perfectly dry powder. He then weighed them, and found the residuum of the phosphorated natron somewhat lighter than that of the vitriolated natron; whence it is probable the former contains the greater quantity of water of crystallization. He has found, that each of the neutral salts which produce any remarkable degree of cold by solution in the mineral acids, viz. phosphorated natron, vitriolated natron, and vitriolated magnesia, lose this property entirely, when deprived by any means of their water of crystallization.

A short time after he had first succeeded in freezing water in summer, by one mixture composed of 3 different salts in water (having been induced to try the effect of such a method, from the consideration that water, already saturated with one kind of salt, will dissolve a portion of another, and after that a 3d, or even more,) he met with the account of an experiment made by M. Homberg, related in one of the earlier volumes of the Philos. Trans. in which it is said he produced an extraordinary degree of cold, by pouring a pint and a half of distilled vinegar on 2 lb. of a powder composed of equal parts of crude sal ammoniac and corrosive sublimate, and shaking them well together. Mr. W. immediately (July 30, 1786) prepared a mixture of this kind in smaller quantity, but found it produced only 32° of cold, the temperature of the air and materials before mixing being 63°; which is no more than he had found may be effected by a solution in water of crude sal ammoniac alone, previously dried and powdered.

By a trial made with great accuracy, he found, that even the mixture composed of diluted vitriolic acid and vitriolated natron is adequate to any useful purpose that may be required in the hottest country; for, by adding 11 parts of the salt in fine powder to 8 parts of the vitriolic acid diluted with an equal weight of water, the thermometer sunk from 80°, the mean temperature of the hottest climate, and to which these materials were purposely heated before mixing, to rather below 20°. Vitriolated natron, added to the marine acid undiluted, produces very nearly as great a degree of cold as when mixed with the diluted nitrous acid. At the temperature of 50°, 2 parts of the acid require 3 parts of the salt in fine powder, which will sink the thermometer to 0°; and if 3 parts of a mixed powder, containing equal parts of muriated ammonia and nitrated kali, be added afterwards, the cold of the mixture will be increased a few degrees more.

The frigorific mixture above described, composed of phosphorated natron and nitrated ammonia dissolved in the diluted nitrous acid, being the most powerful, it will probably be found most convenient for freezing mercury, when snow is not to be procured. The materials for this purpose may be previously cooled in

mixtures made of marine acid with vitriolated natron, muriated ammonia, and nitrated kali, in the proportions mentioned above, this being much cheaper than those made with diluted nitrous acid, and very nearly equal in effect. In his last paper Mr. W. mentioned a freezing mixture, made by dissolving a powder composed of equal parts of muriated ammonia and nitrated kali in water, and therein directed 6 parts of the mixed powder to be added to 8 parts of water; but he has found since, that the best proportions are, 5 parts of the former to 8 of the latter, by which he has sunk the thermometer from 50° to 11°.

Having now prosecuted his subject relative to mixtures for generating artificial cold without the use of ice, from a possible method proposed by Dr. Watson (Essays, vol. 3, p. 139,) for freezing water in summer in this climate, and carried it on to a certain method of freezing, not only water, but even mercury, in the hottest climate, Mr. W. takes his leave of it.

XX. Catalogue of a Second Thousand of New Nebulæ and Clusters of Stars; with a few Introductory Remarks on the Construction of the Heavens. By Wm. Herschel, LL. D, F. R. S. p. 212.

By the continuation of a review of the heavens with a 20 feet reflector, Dr. H. was now furnished with a 2d 1000 of new nebulæ. The form of this work is exactly that of the former part, the classes and numbers being continued, and the same letters used to express, in the shortest way, as many essential features of the objects as could possibly be crowded into so small a compass as that to which I thought it expedient to limit myself. The method I have taken of analyzing the heavens, as it were, is perhaps the only one by which we can arrive at a knowledge of their construction. In the prosecution of so extensive an undertaking, it may well be supposed that many things must have been suggested, by the great variety in the order, the size, and the compression of the stars, as they presented themselves to my view, which it will not be improper to communicate.

To begin our investigation according to some order, let us depart from the objects immediately around us to the most remote that our telescopes, of the greatest power to penetrate into space, can reach. We shall touch but slightly on things that have already been remarked. From the earth, considered as a planet, and the moon as its satellite, we pass through the region of the rest of the planets, and their satellites. The similarity between all these bodies is sufficiently striking to allow us to comprehend them under one general definition, of bodies not luminous in themselves, revolving round the sun. The great diminution of light, when reflected from such bodies, especially when they are also at a great distance from the light which illuminates them, precludes all possibility of following them a great way into space. But if we did not know that

light diminishes as the squares of the distances increase, and that in every reflection a very considerable part is entirely lost, the motion of comets, by which the space through which they run is measured out to us, while on their return from the sun we see them gradually disappear as they advance towards their aphelia, would be sufficient to convince us that bodies shining only with borrowed light can never be seen at any very great distance. This consideration brings us back to the sun, as a refulgent fountain of light, while it establishes at the same time beyond a doubt that every star must likewise be a sun, shining by its own native brightness. Here then we come to the more capital parts of the great construction.

These suns, every one of which is probably of as much consequence to a system of planets, satellites, and comets, as our own sun, are now to be considered, in their turn, as the minute parts of a proportionally greater whole. I need not repeat that by my analysis it appears, that the heavens consist of regions where suns are gathered into separate systems, and that the catalogues I have given comprehend a list of such systems; but may we not hope that our knowledge will not stop short at the bare enumeration of phenomena capable of giving us so much instruction? Why should we be less inquisitive than the natural philosopher, who sometimes, even from an inconsiderable number of specimens of a plant, or an animal, is enabled to present us with the history of its rise, progress, and decay? Let us then compare together, and class some of these numerous sidereal groups, that we may trace the operations of natural causes as far as we can perceive their agency. The most simple form, in which we can view a sidereal system, is that of being globular. This also, very favourably to our design, is that which has presented itself most frequently, and of which I have given the greatest collection.

But first of all it will be necessary to explain what is our idea of a cluster of stars, and by what means we have obtained it. For an instance, I shall take the phenomenon which presents itself in many clusters: it is that of a number of lucid spots, of equal lustre, scattered over a circular space, in such a manner as to appear gradually more compressed towards the middle; and which compression, in the clusters to which I allude, is generally carried so far as, by imperceptible degrees, to end in a luminous centre, of a resolvable blaze of light. To solve this appearance, it may be conjectured that stars of any given, very unequal magnitudes, may easily be so arranged, in scattered, much extended, irregular rows, as to produce the above described picture; or, that stars, scattered about almost promiscuously within the frustrum of a given cone, may be assigned of such properly diversified magnitudes as also to form the same picture. But who, that is acquainted with the doctrine of chances, can seriously maintain such improbable conjectures? To consider this only in a very coarse

way, let us suppose a cluster to consist of 5000 stars, and that each of them may be put into one of 5000 given places, and have one of 5000 assigned magnitudes. Then, without extending our calculation any further, we have five and twenty millions of chances, out of which only one will answer the above improbable conjecture, while all the rest are against it. When we now remark that this relates only to the given places within the frustrum of a supposed cone, whereas these stars might have been scattered all over the visible space of the heavens; that they might have been scattered, even within the supposed cone, in a million of places different from the assumed ones, the chance of this apparent cluster not being a real one, will be rendered so highly improbable that it ought to be entirely rejected.

Mr. Michell computes, (Phil. Trans. vol. 57, p. 246,) with respect to the 6 brightest stars of the Pleiades only, that the odds are near 500000 to 1, that no 6 stars, out of the number of those which are equal in splendour to the faintest of them, scattered at random in the whole heavens, would be within so small a distance from each other as the Pleiades are. Taking it then for granted that the stars which appear to be gathered together in a group, are in reality thus accumulated, I proceed to prove also that they are nearly of an equal magnitude.

The cluster itself, on account of the small angle it subtends to the eye, we must suppose to be very far removed from us. For, were the stars which compose it at the same distance from one another as Sirius is from the sun; and supposing the cluster to be seen under an angle of 10 minutes, and to contain 50 stars in one of its diameters, we should have the mean distance of such stars 12 seconds; and therefore the distance of the cluster from us about 17,000 times greater than the distance of Sirius. Now, since the apparent magnitude of these stars is equal, and their distance from us is also equal,—because we may safely neglect the diameter of the cluster, which, if the centre be 17,000 times the distance of Sirius from us, will give us 17,025 for the farthest, and 17,000 wanting 25 for the nearest star of the cluster;—it follows that we must either give up the idea of a cluster, and recur to the above refuted supposition, or admit the equality of the stars that compose these clusters. It is to be remarked that we do not mean entirely to exclude all variety of size; for the very great distance, and the consequent smallness of the component clustering stars, will not permit us to be extremely precise in the estimation of their magnitudes; though we have certainly seen enough of them to know that they are contained within pretty narrow limits; and do not perhaps exceed each other in magnitude more than in some such proportion as one full-grown plant of a certain species may exceed another full-grown plant of the same species.

If we have drawn proper conclusions relating to the size of stars, we may with still greater safety speak of their relative situations, and affirm that in the same

distances from the centre an equal scattering takes place. If this were not the case, the appearance of a cluster could not be uniformly increasing in brightness towards the middle, but would appear nebulous in those parts which were more crowded with stars; but, as far as we can distinguish, in the clusters of which we speak, every concentric circle maintains an equal degree of compression, as long as the stars are visible; and when they become too crowded to be distinguished, an equal brightness takes place, at equal distances from the centre, which is the most luminous part.

The next step in my argument will be to show that these clusters are of a globular form. This again we rest on the sound doctrine of chances. Here, by way of strength to our argument, we may be allowed to take in all round nebulæ, though the reasons we have for believing that they consist of stars have not as yet been entered into. For, what I have to say concerning their spherical figure will equally hold good whether they be groups of stars or not. In my catalogues we have, I suppose, not less than 1000 of these round objects. Now whatever may be the shape of a group of stars, or of a nebula, which we would introduce instead of the spherical one, such as a cone, an ellipsis, a spheroid, a circle or a cylinder, it will be evident that out of 1000 situations, which the axes of such forms may have, there is but one that can answer the phenomenon for which we want to account; and that is, when those axes are exactly in a line drawn from the object to the place of the observer. Here again we have a million of chances of which all but one are against any other hypothesis than that which we maintain, and which, for this reason, ought to be admitted.

The last thing to be inferred from the above related appearances is, that these clusters of stars are more condensed towards the centre than at the surface. If there should be a group of stars in a spherical form, consisting of such as were equally scattered over all the assigned space, it would not appear to be very gradually more compressed and brighter in the middle; much less would it seem to have a bright nucleus in the centre. A spherical cluster of an equal compression within, for that such there are will be seen hereafter,—may be distinguished by the degrees of brightness which take place in going from the centre to the circumference. Thus, when a is the brightness in the centre, it will be $\sqrt{(a^2 - x^2)}$ at any other distance x from the centre. Or, putting $a = 1$, and $x =$ any decimal fraction; then, in a table of natural sines, where x is the sine, the brightness at x will be expressed by the cosine. Now as a gradual increase of brightness does not agree with the degrees calculated from a supposition of an equal scattering, and as the cluster has been proved to be spherical, it must needs be admitted that there is indeed a greater accumulation towards the centre. And thus, from the above-mentioned appearances, we come to know that there are globular clusters of stars nearly equal in size, which are scattered evenly at

equal distances from the middle, but with an increasing accumulation towards the centre.

We may now venture to raise a superstructure on the arguments that have been drawn from the appearance of clusters of stars and nebulæ of the form we have been examining, which is that of which I have made mention in my Theoretical View—Formation of Nebulæ—Form I, Phil. Trans. vol. 75, p. 214. It is to be remarked that when I wrote the paragraph referred to, I delineated nature as well as I do now; but, as I there gave only a general sketch, without referring to particular cases, what I then delivered may have been considered as little better than hypothetical reasoning, whereas in the present instance this objection is entirely removed, since actual and particular facts are brought to vouch for the truth of every inference.

Having then established that the clusters of stars of the 1st form, and round nebulæ, are of a spherical figure, I think myself plainly authorized to conclude that they are thus formed by the action of central powers. To manifest the validity of this inference, the figure of the earth may be given as an instance; whose rotundity, setting aside small deviations, the causes of which are well known, is without hesitation allowed to be a phenomenon decisively establishing a centripetal force. Nor do we stand in need of the revolving satellites of Jupiter, Saturn, and the Georgium Sidus, to assure us that the same powers are likewise lodged in the masses of these planets. Their globular figure alone must be admitted as a sufficient argument to render this point incontrovertible. We also apply this inference with equal propriety to the body of the sun, as well as to that of Mercury, Venus, Mars, and the moon; as owing their spherical shape to the same cause. And how can we avoid inferring, that the construction of the clusters of stars, and nebulæ likewise, of which we have been speaking, is as evidently owing to central powers? Besides, the step that I here make in my inference is in fact a very easy one, and such as ought freely to be granted. Have I not already shown that these clusters cannot have come to their present formation by any random scattering of stars? The doctrine of chance, by exposing the very great odds against such hypotheses, may be said to demonstrate that the stars are thus assembled by some power or other. Then what do I attempt more than merely to lead the mind to the conditions under which this power is seen to act?

In a case of such consequence I may be permitted to be a little more diffuse, and draw additional arguments from the internal construction of spherical clusters and nebulæ. If we find that there is not only a general form, which, as has been proved, is a sufficient manifestation of a centripetal force, what shall we say when the accumulated condensation, which every where follows a direction towards a centre, is even visible to the very eye? Were we not already ac-

quainted with attraction, this gradual condensation would point out a central power, by the remarkable disposition of the stars tending towards a centre. In consequence of this visible accumulation, whether it may be owing to attraction only, or whether other powers may assist in the formation, we ought not to hesitate in ascribing the effect to such as are central; no phenomena being more decisive in that particular, than those of which I am treating.

I am fully aware of the consequences I shall draw on myself in but mentioning other powers that might contribute to the formation of clusters. A mere hint of this kind, it will be expected, ought not to be given without sufficient foundation; but let it suffice at present to remark that my arguments cannot be affected by my terms: whether I am right to use the plural number, —central powers,—or whether I ought only to say,—the known central force of gravity,—my conclusions will be equally valid. I will however add, that the idea of other central powers being concerned in the construction of the sidereal heavens, is not one that has only lately occurred to me. Long ago I have entertained a certain theory of diversified central powers of attractions and repulsions; an exposition of which I have even delivered in the years 1780 and 1781, to the Philosophical Society then existing at Bath, in several mathematical papers on that subject. I shall however set aside an explanation of this theory, which would not only exceed the intended limits of this paper, but is moreover not required for what remains at present to be added, and therefore may be given some other time, when I can enter more fully into the subject of the interior construction of sidereal systems. To return, then, to the case immediately under our present consideration, it will be sufficient that I have abundantly proved that the formation of round clusters of stars and nebulæ is either owing to central powers, or at least to one such force as refers to a centre.

I shall now extend the weight of my argument, by taking in likewise every cluster of stars or nebula that shows a gradual condensation, or increasing brightness, towards a centre or certain point; whether the outward shape of such clusters or nebulæ be round, extended, or of any other given form. What has been said with regard to the doctrine of chance, will of course apply to every cluster, and more especially to the extended and irregular shaped ones, on account of their greater size: it is among these that we find the largest assemblages of stars, and most diffusive nebulosities; and therefore the odds against such assemblages happening without some particular power to gather them, increase exceedingly with the number of the stars that are taken together. But if the gradual accumulation either of stars or increasing brightness has before been admitted as a direction to the seat of power, the same effect will equally point out the same cause in the cases now under consideration. There are besides

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some additional circumstances in the appearance of extended clusters and nebulæ, that very much favour the idea of a power lodged in the brightest part. Though the form of them be not globular, it is plainly to be seen that there is a tendency towards sphericity, by the swell of the dimensions the nearer we draw towards the most luminous place, denoting as it were a course, or tide of stars, setting towards a centre. And—if allegorical expressions may be allowed—it should seem as if the stars thus flocking towards the seat of power were stemmed by the crowd of those already assembled, and that while some of them are successful in forcing their predecessors sideways out of their places, others are themselves obliged to take up with lateral situations, while all of them seem equally to strive for a place in the central swelling, and generating spherical figure. Since then almost all the nebulæ and clusters of stars I have seen, the number of which is not less than three and twenty hundred, are more condensed and brighter in the middle; and since, from every form, it is now equally apparent that the central accumulation or brightness must be the result of central powers, we may venture to affirm that this theory is no longer an unfounded hypothesis, but is fully established on grounds which cannot be overturned.

Let us endeavour to make some use of this important view of the constructing cause, which can thus model sidereal systems. Perhaps, by placing before us the very extensive and varied collection of clusters and nebulæ, furnished by my catalogues, we may be able to trace the progress of its operation, in the great laboratory of the universe. If these clusters and nebulæ were all of the same shape, and had the same gradual condensation, we should make but little progress in this inquiry; but, as we find so great a variety in their appearances, we shall be much sooner at a loss how to account for such various phenomena, than be in want of materials on which to exercise our inquisitive endeavours.

Some of these round clusters consist of stars of a certain magnitude, and given degree of compression, while the whole cluster itself takes up a space of perhaps 10 minutes; others appear to be made up of stars that are much smaller, and much more compressed, when at the same time the cluster itself subtends a much smaller angle, such as 5 minutes. This diminution of the apparent size, and compression of stars, as well as diameter of the cluster to 4, 3, 2 minutes, may very consistently be ascribed to the different distances of these clusters from the place in which we observe them; in all which cases we may admit a general equality of the sizes, and compression of the stars that compose them, to take place. It is also highly probable that a continuation of such decreasing magnitudes, and increasing compression, will justly account for the appearance of round, easily resolvable, nebulæ; where there is almost a certainty of their being clusters of stars. And no astronomer can hesitate to go

still farther, and extend his surmises by imperceptible steps to other nebulæ, that still preserve the same characteristics, with the only variations of vanishing brightness, and reduction of size.

Other clusters there are that, when they come to be compared with some of the former, seem to contain stars of an equal magnitude, while their compression appears to be considerably different. Here the supposition of their being at different distances will either not explain the apparently greater compression, or, if admitted to do this, will convey to us a very instructive consequence: which is, that the stars which are thus supposed not to be more compressed than those in the former cluster, but only to appear so on account of their greater distance, must needs be proportionally larger, since they do not appear of less magnitude than the former. As therefore one or other of these hypotheses must be true, it is not at all improbable but that, in some instances, the stars may be more compressed; and in others, of a greater magnitude. This variety of size, in different spherical clusters, I am however inclined to believe may not go further than the difference in size, found among the individuals belonging to the same species of plants, or animals, in their different states of age, or vegetation, after they are come to a certain degree of growth. A further inquiry into the circumstance of the extent, both of condensation and variety of size, that may take place with the stars of different clusters, we shall postpone till other things have been previously discussed.

Let us then continue to turn our view to the power which is moulding the different assortments of stars into spherical clusters. Any force, that acts uninterruptedly, must produce effects proportional to the time of its action. Now, as it has been shown that the spherical figure of a cluster of stars is owing to central powers, it follows that those clusters which, ceteris paribus, are the most complete in this figure, must have been the longest exposed to the action of these causes. This will admit of various points of views. Suppose for instance that 5000 stars had been once in a certain scattered situation, and that other 5000 equal stars had been in the same situation; then that of the two clusters which had been longest exposed to the action of the modelling power, we suppose, would be most condensed, and more advanced to the maturity of its figure. An obvious consequence that may be drawn from this consideration is, that we are enabled to judge of the relative age, maturity, or climax of a sidereal system, from the disposition of its component parts; and, making the degrees of brightness in nebulæ stand for the different accumulation of stars in clusters, the same conclusions will extend equally to them all. But we are not to conclude, from what has been said, that every spherical cluster is of an equal standing in regard to absolute duration, since one that is composed of a thousand stars only, must certainly arrive to the perfection of its form sooner than

another which takes in a range of a million. Youth and age are comparative expressions; and an oak of a certain age may be called very young, while a cotemporary shrub is already on the verge of its decay. The method of judging with some assurance of the condition of any sidereal system, may perhaps not improperly be drawn from the standard before laid down page 589; so that, for instance, a cluster or nebula which is very gradually more compressed and bright towards the middle, may be in the perfection of its growth, when another which approaches to the condition pointed out by a more equal compression, such as the nebulæ I have called planetary seem to present us with, may be considered as very aged, and drawing on towards a period of change, or dissolution. This has been before surmised, when, in a former paper, I considered the uncommon degree of compression that must prevail in a nebula to give it a planetary aspect; but the argument, which is now drawn from the powers that have collected the formerly scattered stars to the form we find they have assumed, must greatly corroborate that sentiment.

This method of viewing the heavens seems to throw them into a new kind of light. They now are seen to resemble a luxuriant garden, which contains the greatest variety of productions, in different flourishing beds; and one advantage we may at least reap from it is, that we can, as it were, extend the range of our experience to an immense duration. For, to continue the simile I have borrowed from the vegetable kingdom, is it not almost the same thing, whether we live successively to witness the germination, blooming, foliage, fecundity, fading, withering, and corruption of a plant, or whether a vast number of specimens, selected from every stage through which the plant passes in the course of its existence, be brought at once to our view?

Dr. H. then adds the catalogue of the 1000 new nebulæ and clusters of stars, the numbers, dates of observation, names, situations, and several other characteristic circumstances, are arranged in 8 columns of a table, which is divided into 8 classes or collections:—1. The 1st class is of such as are titled, from their appearance in the heavens, bright nebulæ; the 2d class are the faint nebulæ; the 3d class, the very faint nebulæ; the 4th class, planetary nebulæ; the 5th class, very large nebulæ; the 6th class, very compressed, and clusters of stars; the 7th class, pretty much compressed clusters of large or small stars; and the 8th or last class, coarsely scattered clusters of stars. To the catalogue Dr. H. adds the following postscript, to announce a newly discovered satellite of the planet Saturn.

P. S. The planet Saturn has a 6th satellite revolving round it in about 32^h 48^m. Its orbit lies exactly in the plane of the ring, and within that of the 1st satellite. An account of its discovery with the 40 feet reflector, and a more accurate determination of its revolution and distance from the planet, will be presented to the R. S. at their next meetings.

XXI. An Attempt to explain a Difficulty in the Theory of Vision, depending on the Different Refrangibility of Light. By the Rev. Nevil Maskelyne, D. D., F. R. S., &c. p. 256.

The ideas of sight are so striking and beautiful, that we are apt to consider them as perfectly distinct. The celebrated Euler, taking this for granted, has supposed, in the Memoirs of the Royal Academy of Sciences at Berlin for 1747, that the several humors of the human eye were contrived in such a manner as to prevent the latitude of focus arising from the different refrangibility of light, and considers this as a new reason for admiring the structure of the eye; for that a single transparent medium, of a proper figure, would have been sufficient to represent images of outward objects in an imperfect manner; but, to make the organ of sight absolutely complete, it was necessary it should be composed of several transparent mediums, properly figured, and fitted together agreeable to the rules of the sublimest geometry, in order to obviate the effect of the different refrangibility of light in disturbing the distinctness of the image; and hence he concludes, that it is possible to dispose 4 refracting surfaces in such a manner as to bring all sorts of rays to one focus, at whatever distance the object be placed. He then assumes a certain hypothesis of refraction of the differently refrangible rays, and builds on it an ingenious theory of an achromatic object-glass, composed of 2 meniscus glasses with water between them, with the help of an analytical calculation, simple and elegant, as his usually are.

He has not however demonstrated the necessary existence of his hypothesis, his arguments for which are more metaphysical than geometrical; and as it was founded on no experiment, so those made since have shown its fallacy, and that it does not obtain in nature. Also, which is rather extraordinary, it does not account, according to his own ideas, for the very phenomenon which first suggested it to him, namely, the great distinctness of the human vision, as was observed to Dr. M. many years ago, by the late Mr. John Dollond, F. R. S. to whom we are so much obliged for the invention of the achromatic telescope; for the refractions at the several humors of the eye being all made one way, the colours produced by the first refraction will be increased at the 2 subsequent ones, instead of being corrected, whether we make use of Newton's or Euler's law of refraction of the differently refrangible rays.

Thus Euler produced an hypothetical principle, neither fit for rendering a telescope achromatic, nor to account for the distinctness of the human vision; and the difficulty of reconciling that distinctness with the principle of the different refrangibility of light, discovered by Sir Isaac Newton, remains in its full force. In order to go to the bottom of this difficulty, as the best probable means of obviating it, Dr. M. calculated the refractions of the mean, most

and least refrangible rays, at the several humors of the eye, and thence inferred the diffusion of the rays, proceeding from a point in an object, at their falling on the retina, and the external angle that such coloured image of a point upon the retina corresponds to.

He took the dimensions of the eye from M. Petit, as related by Dr. Jurin; and the specific gravities of the aqueous and vitreous humors having been found to be nearly the same with that of water, and the refraction of the vitreous humor of an ox's eye having been found by Mr. Hauksbee to be the same as that of water, and the ratio of refraction out of air into the crystalline humor of an ox's eye having been found by the same accurate experimenter to be as 1 to .68327, he took the refraction of the mean refrangible rays out of air into the aqueous or vitreous humor, the same as into water, as 1 to .74853, or 1.33595 to 1; and out of air into the crystalline humor as 1 to .68327, or 1.46355 to 1. Hence he found, according to Sir Isaac Newton's 2 theorems, related at part 2, book 1 of Optics, p. 113, that the ratio of refraction of the most, mean, and least refrangible rays at the cornea, should be as 1 to .74512, .74853 and .75197; at the fore surface of the crystalline as 1 to .91173, .91282, and .91392; and at the hinder surface of the crystalline as 1 to 1.09681, 1.09550, and 1.09420. Now taking, with Dr. Jurin, 15 inches for the distance at which the generality of eyes in their mean state see with most distinctness, he found the rays from a point of an object so situated, will be collected into 3 several foci, viz. the most, mean, and least refrangible rays, at the respective distances behind the crystalline, .5930, .6034, and .6141 of an inch, the focus of the most refrangible rays being .0211 inch short of the focus of the least refrangible ones.

Also, assuming the diameter of the pencil of rays at the cornea, proceeding from the object at 15 inches distance, to be $\frac{1}{5}$ of an inch in a strong light, which is a large allowance for it, the semi-angle of the pencil of mean refrangible rays at their concourse on the retina will be 7° 12′, whose tangent to the radius unity, or .1264, multiplied into .0211 inch, the interval of the foci of the extreme refrangible rays, gives .002667 inch for the diffusion of the different coloured rays, or the diameter of the indistinct circle on the retina. Now, having found that the diameter of the image of an object on the retina, is to the object, as .6055 inch, to the distance of the object from the centre of curvature of the cornea; or the size of the image is the same as would be formed by a very thin convex lens, whose focal distance is .6055 inch; and consequently a line in an object which subtends an angle of 1′ at the centre of the cornea, will be represented on the retina by a line of $\frac{1}{5678}$ inch. Hence the diameter of the indistinct circle on the retina before found, .002667, will answer to an external angle of .002667 × 5678′ = 15′ 8″, or every point in an

object should appear to subtend an angle of about 15′, on account of the different refrangibility of the rays of light.

Dr. M. now shows that this angle of ocular aberration is compatible with the distinctness of our vision. This aberration is of the same kind as that which we experience in the common refracting telescope. Now, by computation from the tabular apertures and magnifying powers of such telescopes, it is certain that they admit of an angular indistinctness at the eye of no less than 57′; therefore the ocular aberration is near 4 times less than in a common refracting telescope, and consequently the real indistinctness, being as the square of the angular aberration, will be 14 or 15 times less in the eye than in a common refracting telescope, which may be easily allowed to be imperceptible.

Further, Sir Isaac Newton has observed, with respect to the like difficulty of accounting for the distinctness with which refracting telescopes represent objects, that the erring rays are not scattered uniformly over the circle of dissipation in the focus of the object-glass, but collected infinitely more densely in the centre than in any other part of the circle, and in the way from the centre to the circumference become continually rarer and rarer, so as at the circumference to become infinitely rare; and by reason of their rarity are not strong enough to be visible, unless in the centre and very near it. He further observes, that the most luminous of the prismatic colours are the yellow and orange, which affect the sense more strongly than all the rest together; and next to these in strength are the red and green; and that the blue, indigo, and violet, compared with these, are much darker and fainter, and compared with the other stronger colours, little to be regarded; and that therefore the images of the objects are to be placed not in the focus of the mean refrangible rays, which are in the confine of green and blue, but in the middle of the orange and yellow, there where the colour is most luminous, that which is in the brightest yellow, that yellow which inclines more to orange than to green.

From all these considerations, and by an elaborate calculation, he infers, that though the whole breadth of the image of a lucid point be $\frac{1}{55}$th of the diameter of the aperture of the object-glass, yet the sensible image of the same is scarce broader than a circle whose diameter is $\frac{1}{250}$th part of the diameter of the aperture of the object-glass of a good telescope; and hence he accounts for the apparent diameters of the fixed stars as observed with telescopes by astronomers, though in reality they are but points.

The like reasoning is applicable to the circle of dissipation on the retina of the human eye; and therefore we may lessen the angular aberration, before computed at 15′, in the ratio of 250 to 55, which will reduce it to 3′ 18″. This reduced angle of aberration may perhaps be double the apparent diameter of the brightest fixed stars to an eye disposed for seeing most distinctly by parallel rays;

or, if short-sighted, assisted by a proper concave lens; which may be thought a sufficient approximation in an explication grounded on a dissipation of rays, to which a precise limit cannot be assigned, on account of the continual increase of density from the circumference to the centre. Certainly some such angle of aberration is necessary to account for the stars appearing under any sensible angle to such an eye; and if we were, without reason, to suppose the images on the retina to be perfect, we should be put to a much greater difficulty to account for the fixed stars appearing otherwise than as points, than we have now been to account for the actual distinctness of our sight. The less apparent diameter of the smaller fixed stars agrees also with the theory; for the less luminous the circle of dissipation is, the nearer we must look towards its centre to find rays sufficiently dense to move the sense. From Sir Isaac Newton's geometrical account of the relative density of the rays in the circle of dissipation, given in his system of the world, it may be inferred, that the apparent diameters of the fixed stars, as depending on this cause, are nearly as their whole quantity of light.

In further elucidation of this subject Dr. M. adds his own experiment. When he looked at the brighter fixed stars, at considerable elevations, through a concave glass fitted, as he is short-sighted, to show them with most distinctness, they appeared to him without scintillation, and as a small round circle of fire of a sensible magnitude. When he looked at them without the concave glass, or with one not suited to his eye, they appeared to cast out rays of a determinate figure, not exactly the same in both eyes, somewhat like branches of trees, which doubtless arise from something in the construction of the eye, and to scintillate a little, if the air be not very clear. To see day objects with most distinctness, he requires a less concave lens by 1 degree, than for seeing the stars best by night; the cause of which seems to be, that the bottom of the eye being illuminated by the day objects, and so rendered a light ground, obscures the fainter colours, blue, indigo, and violet in the circle of dissipation, and therefore the best image of the object will be found in the focus of the bright yellow rays, and not in that of the mean refrangible ones, or the dark green, agreeable to Newton's remark, and consequently nearer the retina of a short-sighted person; but the parts of the retina surrounding the circle of dissipation of a star being in the dark, the fainter colours, blue, indigo, and violet, will have some share in forming the image, and consequently the focus will be shorter.

The apparent diameter of the stars here accounted for, is different from that explained by Dr. Jurin, in his Essay on distinct and indistinct vision, arising from the natural constitution of the generality of eyes to see objects most distinct at moderate distances, and few being capable of altering their conformation enough to see distant objects, and among them the celestial ones, with equal distinctness. But the cause of error, which is here pointed out, will affect

all eyes, even those which are adapted to distant objects. If this attempt to show the compatibility of the actual distinctness of our sight with the different refrangibility of light be admitted as just and convincing, we shall have fresh reason to admire the wisdom of the Creator, in so adapting the aperture of the pupil and the different refrangibility of light to each other, as to render the picture of objects on the retina relatively, though not absolutely, perfect, and fitted for every useful purpose; "where," to borrow the words of our religious and oratorical philosopher Derham, "all the glories of the heavens and earth are brought and exquisitely pictured."

Nor does it appear, that any material advantage would have been obtained, if the image of objects on the retina had been made absolutely perfect, unless the acuteness of the optic nerve should have been increased at the same time; as the minimum visible depends no less on that circumstance than the other. But that the sensibility of the optic nerve could not have been much increased beyond what it is, without great inconvenience to us, may be easily conceived, if we only consider the forcible impression made on our eyes by a bright sky, or even the day objects illuminated by a strong sun. Hence we may conclude, that such an alteration would have rendered our sight painful instead of pleasant, and noxious instead of useful. We might indeed have been enabled to see more in the starry heavens with the naked eye, but it must have been at the expense of our daily labours and occupations, the immediate and necessary employment of man.

To obviate an objection to the diffusion of the rays on the retina by the different refrangibility of light, it may be said, that the ocular aberration, being a separate cause from any effect of the telescope, should subsist equally when we observe a star through a telescope as when we look at it with the naked eye; and that therefore the fixed stars could not appear so small as they have been found to do through the best telescopes, and particularly by Dr. Herschel with his excellent ones. To this Dr. M. answers, that the ocular aberration, which is proportional to the diameter of the pupil when we use the naked eye, is proportional to the diameter of the pencil of rays at the eye when we look through a telescope, which being many times less than that of the pupil itself, the ocular aberration will be diminished in proportion, and become insensible.

XXII. Experiments and Observations on Electricity. By Mr. William Nicholson. p. 265.

Mr. N. divides this paper into 3 sections. 1st. On the excitation of electricity; 2d, On the luminous appearances of electricity, and the action of points; 3d, Of compensated electricity.

1. A glass cylinder was mounted, and a cushion applied with a silk flap, pro-

ceeding from the edge of the cushion over its surface, and thence half round the cylinder. The cylinder was then excited by applying an amalgamed leather in the usual manner. The electricity was received by a conductor, and passed off in sparks to Lane's electrometer. By the frequency of these sparks, or by the number of turns required to cause spontaneous explosion of a jar, the strength of the excitation was ascertained.—2. The cushion was withdrawn about 1 inch from the cylinder, and the excitation performed by the silk only. A stream of fire was seen between the cushion and the silk; and much fewer sparks passed between the balls of the electrometer.—3. A roll of dry silk was interposed, to prevent the stream from passing between the cushion and the silk. Very few sparks then appeared at the electrometer.—4. A metallic rod, not insulated, was then interposed, instead of the roll of silk, so as not to touch any part of the apparatus. A dense stream of electricity appeared between the rod and the silk, and the conductor gave many sparks.—5. The knob of a jar being substituted in the place of the metallic rod, it became charged negatively.—6. The silk alone, with a piece of tin foil applied behind it, afforded much electricity, though less than when the cushion was applied with a light pressure. The hand, being applied to the silk as a cushion, produced a degree of excitation seldom equalled by any other cushion.—7. The edge of the hand answered as well as the palm.—8. When the excitation by a cushion was weak, a line of light appeared at the anterior part of the cushion, and the silk was strongly disposed to receive electricity from any uninsulated conductor. These appearances did not obtain when the excitation was by any means made very strong.—9. A thick silk, or 2 or more folds of silk, excited worse than a single very thin flap. He used the silk called Persian.—10. When the silk was separated from the cylinder, sparks passed between them; the silk was found to be in a weak negative, and the cylinder in a positive state.

The foregoing experiments show, that the office of the silk is not merely to prevent the return of electricity from the cylinder to the cushion, but that it is the chief agent in the excitation; while the cushion serves only to supply the electricity, and perhaps increase the pressure at the entering part. There seems also to be little reason to doubt but that the disposition of the electricity to escape from the surface of the cylinder is not prevented by the interposition of the silk, but by a compensation after the manner of a charge; the silk being then as strongly negative as the cylinder is positive: and, lastly, that the line of light between the silk and cushion in weak excitations does not consist of returning electricity, but of electricity which passes to the cylinder, in consequence of its not having been sufficiently supplied, during its contact with the rubbing surface.

11. When the excitation was very strong in a cylinder newly mounted, flashes of light were seen to fly across its inside, from the receiving surface to the sur-

face in contact with the cushion, as indicated by the brush figure. These made the cylinder ring, as if struck with a bundle of small twigs. They seem to have arisen from part of the electricity of the cylinder taking the form of a charge.

12. With a view to determine what happens in the inside of the cylinder, recourse was had to a plate machine. One cushion was applied with its silken flap. The plate was 9 inches in diameter and $\frac{2}{10}$ of an inch thick. During the excitation, the surface opposite the cushion strongly attracted electricity, which it gave out when it arrived opposite the extremity of the flap. So that a continual stream of electricity passed through an insulated metallic bow terminating in balls, which were opposed, the one to the surface opposite the extremity of the silk, and the other opposite the cushion; the former ball showing positive, and the latter negative signs. The knobs of 2 jars being substituted in the place of these balls, the jar, applied to the surface opposed to the cushion, was charged negatively, and the other positively. This disposition of the back surface seemed, by a few trials, to be weaker the stronger the action of the cushion, as judged by the electricity on the cushion side.—Hence it follows, that the internal surface of a cylinder is so far from being disposed to give out electricity during the friction by which the external surface acquires it, that it even greedily attracts it.

13. A plate of glass was applied to the revolving plate, and thrust under the cushion in such a manner as to supply the place of the silk flap. It rendered the electricity stronger, and appears to be an improvement of the plate machine; to be admitted if there were not essential objections against the machine itself.

14. Two cushions were then applied on the opposite surfaces with their silk flaps, so as to clasp the plate between them. The electricity was received from both by applying the finger and thumb to the opposite surfaces of the plate. When the finger was advanced a little towards its correspondent cushion, so that its distance was less than between the thumb and its cushion, the finger received strong electricity, and the thumb none; and, contrariwise, if the thumb were advanced beyond the finger, it received all the electricity, and none passed to the finger. This electricity was not stronger than was produced by the good action of one cushion applied singly.

15. The cushion in experiment 12 gave most electricity when the back surface was supplied, provided that surface was suffered to retain its electricity till the rubbed surface had given out its electricity.

From the last 2 paragraphs it appears, that no advantage is gained by rubbing both surfaces; but that a well managed friction on one surface will accumulate as much electricity as the present methods of excitation seem capable of collecting; but that when the excitation is weak, on account of the electric matter not passing with sufficient facility to the rubbed surface, the friction enables the op-

posite surface to attract or receive it, and if it be supplied, both surfaces will pass off in the positive state; and either surface will give out more electricity than is really induced on it, because the electricity of the opposite surface forms a charge. For the substance of the remainder of this paper, reference may be had to Mr. Nicholson's ingenious Introduction to Natural Philosophy, in 2 vols. 8vo.

XXIII. Experiments on the Transmission of the Vapour of Acids through a Hot Earthen Tube, and further Observations relating to Phlogiston. By the Rev. Joseph Priestley, LL. D., F. R. S. p. 289.

In Dr. P's former experiments on the phlogistication of spirit of nitre by heat it appeared, that when pure air was expelled from what is called dephlogisticated spirit of nitre, the remainder was left phlogisticated. This he found abundantly confirmed by repeating the experiments in a different manner, and on a larger scale; and he applied the same process to other acids and liquors of a different kind. From these it will appear, that oil of vitriol and spirit of nitre, in their most dephlogisticated state, consist of a proper saturation of the acids with phlogiston, so that what we have called the phlogistication of them, ought rather to have been called their super-phlogistication.

He began with treating a quantity of oil of vitriol as he had done the spirit of nitre, viz. exposing it to heat in a glass tube, hermetically sealed, and nearly exhausted; and the result was similar to that of the experiment with the nitrous acid, with respect to the expulsion of air from it, though, the phlogistication not appearing by any change of colour, I did not in this method ascertain that circumstance. The particulars were as follow. After the acid had been made to boil some time, a dense white vapour appeared in quick motion at a distance above the acid; and though, on withdrawing the fire, that vapour disappeared, it instantly re-appeared on renewing the heat. When the tube was cool, he opened it under water, and a quantity of air rushed out, though the acid had been made to boil violently while it was closing, so that there could not have been much air in the tube. This air, which must therefore have been generated in the tube, was a little worse than common air, being of the standard of 1.12 when the latter was 1.04.

That this air should be worse than common air, Dr. P. could not well explain. But in his former experiments it appeared that vitriolic acid air injures common air; and that in proportion as pure air is expelled from this acid, the remainder becomes phlogisticated, or charged with vitriolic acid air, clearly appeared in the following experiment. Making a quantity of oil of vitriol boil in a glass retort, and making the vapour pass through a red-hot earthen tube, glazed inside and out, and filled with pieces of broken tubes, he collected the liquor that distilled

over, and found it to be the same thing with water impregnated with vitriolic acid air. The smell of it was exceedingly pungent; and it was evident, that more of this air had escaped than could be retained by that quantity of water. The oil of vitriol used in this process was 1 oz. 9 dw. 18 gr. and the liquor collected was 6 dw. 12 gr. When he collected the air that was produced in this manner, it appeared to be very pure, about the standard of 0.3 with 2 equal measures of nitrous air. At another time, expending 1 oz. 11 dw. 18 gr. of oil of vitriol, of the specific gravity of 1856, that of water being 1000, he collected 19 dw. 6 gr. of the volatile acid, of the specific gravity of 1340, and 130 oz. measures of dephlogisticated air of the purest kind, viz. of the standard of 0.15. Going through the same process with spirit of nitre, the result was in all respects similar, but much more striking, the production of both dephlogisticated air and phlogisticated acid vapour being prodigiously quicker, and more abundant. Expending 5 oz. 8 dw. 6 gr. of spirit of nitre, he collected 600 oz. measures of very pure dephlogisticated air, being of the standard of 0.2. He also collected 1 oz. 7 dw. 14 gr. of a greenish acid of nitre, which emitted copious red fumes. All the apparatus beyond the hot tube was filled with the densest red vapour, and the water of the trough in which the air was received was so much impregnated with it, that the smell was very strong; and it spontaneously yielded nitrous air several days, just as water does when impregnated with nitrous vapour. Perceiving the emission of air from the water, after it had stood some time, he filled a jar containing 30 oz. measures with it, and without any heat it yielded 2 oz. measures of the strongest nitrous air.

To try whether the acid, thus supersaturated with phlogiston, was convertible into pure air by this process, Dr. P. heated the liquor collected after the distillation of the oil of vitriol, that is, water impregnated with vitriolic acid air, and made the vapour pass through the hot tube, but no air came from it; and when collected a 2d time, it was not at all different from what it had been before. The specific gravity was also the same. It is evident however, though this process does not show it, that the volatile vitriolic acid contains the proper element of dephlogisticated air; since by melting iron into vitriolic acid air, a quantity of fixed air, which is composed of inflammable and dephlogisticated air, is produced. Melting iron in 9 oz. measures of vitriolic acid air, it was reduced to 0.3 oz. measures, and of this 0.17 oz. measures was fixed air. He repeated the experiment with the same result, and putting the residuums together, found the air to be inflammable.

Though, in the process with spirit of salt, the result be different from that of those with oil of vitriol and spirit of nitre, yet there is an analogy among all these 3 acids in this respect, viz. that the marine and both the volatile acids of vitriol and nitre are made by impregnating water with the acid vapour; so that in

its usual state it may be said to be phlogisticated as well as these. It was evident that the water in the worm-tub was much more heated by the distillation of the spirit of salt than by that of the oil of vitriol, and especially that of the spirit of nitre; so that much of the heat by which it had been raised in vapour must, in the latter case, have been latent in the air that was formed; whereas, in the other case, it was communicated to the water in the worm-tub.

The vapour of dephlogisticated marine acid, which M. Berthollet discovered, and with which water may be impregnated as with fixed air, being made to pass through the hot earthen tube, became dephlogisticated air, as in the following experiment. Having poured a quantity of spirit of salt on some manganese in a glass retort, he heated it as in the preceding experiments with a proper apparatus both for receiving the distilled liquor, and the air. He found $\frac{1}{10}$ of the air was fixed air, and the remainder very pure dephlogisticated. The liquor received in this distillation resembled strong spirit of salt in which manganese had been put. This process immediately succeeding that in which the glass tube, joining the earthen tube and worm-tub, was left full of black matter by the distillation of the alkaline liquor, mentioned hereafter, the blackness presently vanished, and the tube became transparent as before.

Distilled vinegar, submitted to this process, yielded air $\frac{1}{5}$ of which was fixed air, and the rest inflammable: expending 2 oz. 19 dw. 0 gr. of the acid, he got 1 oz. 19 dw. 0 gr. of a liquor which had a more pungent smell than it had before distillation. It had also some black matter in it, and some of the same remained at the bottom of the retort when the liquor was evaporated to dryness. The air received was 90 oz. measures.

Alkaline air is converted into inflammable air in this process, as well as by the electric spark, but by no means in so great a degree. Dr. P. put 2 oz. 10 dwt. of water pretty strongly impregnated with alkaline air into the retort, and heating it, sent the vapour through the hot tube; when he collected 2 oz. 3 dwt. of liquor, which had a disagreeable empyreumatic smell, as well as that of a volatile alkali, and it was quite opaque with a black matter, which subsided to the bottom of the vessel. Also the tube through which the air and vapour had been conveyed was left quite black, as mentioned above.

Dr. P. now recites a few experiments of a different kind from those above-mentioned, and more immediately relating to the doctrine of phlogiston. It is said, by those who do not admit the doctrine of phlogiston, that the metals are simple substances, which, having a strong affinity to dephlogisticated air, imbibe it when they become calces, without parting with any thing. But that something is really parted with in the calcination, as they will call it, of iron in dephlogisticated air, appears to be very evident; as well as in the process with steam. In $6\frac{1}{2}$ oz. measures of dephlogisticated air he melted turnings of malleable iron

till there remained only $1\frac{1}{3}$ oz. measure, and of this $\frac{87}{30}$ oz. measure was fixed air. In 6 oz. measures of dephlogisticated air, of the standard of 0.2, he melted iron till it was reduced to $\frac{2}{3}$ of an ounce measure, of which $\frac{1}{4}$ was fixed air, and the remainder completely phlogisticated. Again, he melted iron in $7\frac{1}{2}$ oz. measures of dephlogisticated air, of the same purity with that in the last experiment, when it was reduced to $1\frac{1}{3}$ oz. measure, and of this $\frac{1}{4}$ was fixed air, and the remainder phlogisticated. In this case Dr. P. carefully weighed the finery cinder that was formed in the process, and found it to be 9 grains, so that the iron that had been melted, being about $\frac{2}{3}$ of this weight, had been about 6 grains.

When the dephlogisticated air is more impure, the quantity of fixed air will always be less in proportion. Thus, having melted iron in 7 oz. measures of dephlogisticated air of the standard of 0.65, it was reduced to 1.6 oz. m.; and of this only $\frac{1}{4}$ of an ounce measure was fixed air. This however is much more than can come from the plumbago in the iron; but as the production of this fixed air is by many ascribed to this plumbago, it may be worth while to show by computation that it is impossible that it should have this origin. Both the quantity of plumbago in iron, and the quantity of fixed air in plumbago, are much too small for the purpose. From half an ounce of the purest plumbago, Dr. P. first got, in a coated glass retort, 13 oz. measures of air, of which only 3 oz. measures were fixed air, the rest being inflammable; then putting it into an earthen tube, he kept it some hours in as great a heat as he could produce, and got 22 oz. m. more; and of this also only 3 were fixed, and the rest inflammable, and the last portion was wholly so.

But instead of supposing the fixed air that he got to be that which was expelled from the plumbago in the iron, he would suppose that even the whole of this plumbago afforded only 1 of the elements of the fixed air, viz. phlogiston, or that which the French chemists call carbone; and that this principle, by its union with the dephlogisticated air in the vessel, forms the fixed air, yet on this most unfavourable and improbable supposition the quantity will be found to be insufficient. If 100 gr. of iron contain, according to M. Bergman, 0.12 gr. of plumbago, 7 gr. would contain only 0.0084 gr. of plumbago; and if we suppose, with Mr. Kirwan, that 100 cubic inches of fixed air contain 8.14 gr. of phlogiston, the fixed air produced in one of the above-mentioned processes (viz. $\frac{1}{4}$ of an ounce measure) would contain .032 gr. of phlogiston, which is above 3 times more than the plumbago in the iron could furnish. It is evident therefore, that the quantity of fixed air that he found must have been formed by phlogiston from the iron uniting with the dephlogisticated air in the vessel.

Another argument against the antiphlogistic doctrine may be drawn from an experiment which Dr. P. made on Prussian blue; if the small quantity of fixed air, that may be expelled from it by heat, be compared with the much greater

quantity which is produced when heated in dephlogisticated air. Prussian blue is generally said to be a calx of iron supersaturated with phlogiston, though of late it has been said by some that it has acquired something that is of the nature of an acid. From his experiments on it, with a burning lens in dephlogisticated air, he infers that the former hypothesis is true, except that the substance contains some fixed air, which is no doubt an acid; for much of the dephlogisticated air disappears, just as in the preceding similar process with iron.

He threw the focus of the burning lens on 2 dwt. 5 gr. of Prussian blue in a vessel of dephlogisticated air, of the standard of 0.53, till all the colour was discharged. Being then weighed, it was 1 dwt. 2 gr. In this process $7\frac{1}{4}$ oz. of fixed air had been produced, and what remained of the air was of the standard of 0.94. Heating the brown powder to which the Prussian blue was reduced in this experiment in inflammable air, it imbibed $8\frac{1}{2}$ oz. m. of it, and became of a black colour; but it was neither attracted by the magnet, nor was it soluble in oil of vitriol and water, as he had expected it would have been. Again, he heated Prussian blue in dephlogisticated air, of the standard of 0.2, without producing any sensible increase of its bulk, when he found 3 oz. measures of it to be fixed air, and the standard of the residuum, with 2 measures of nitrous air, was 1.35. The substance had lost 11 gr., the greatest part of which was evidently water. To determine what quantity of fixed air Prussian blue would yield by mere heat, he put half an ounce of it into an earthen tube, and got from it 56 oz. m. of air, of which 16 oz. m. were fixed air, in the proportion of $\frac{1}{3}$ in the first portion, and $\frac{1}{4}$ in the last. The remainder was inflammable. There remained 5 dwt. 20 gr. of a black powder, with a very little of it (probably the surface) brown.

Comparing these experiments, it will appear, that the fixed air procured by means of Prussian blue and dephlogisticated air, must have been formed by phlogiston from the Prussian blue and the dephlogisticated air in the vessel: for if 240 gr. of this substance yield 16 oz. measures of fixed air, 10 gr. of it, which is more than was used in the experiment, would have yielded only 0.6 oz. m. Nor is it possible to account for the disappearing of so much dephlogisticated air, but on the supposition of its being employed in forming this fixed air.

XXIV. On the Production of Nitrous Acid and Nitrous Air. By the Rev. Isaac Milner, B. D., F. R. S., &c. p. 300.

1. It has been known for some time, that a relation subsists between nitrous acid and volatile alkali. The latter has frequently been produced by help of the former; but Mr. M. does not recollect that, in any instance, the volatile alkali has been proved to contribute to the formation of nitrous acid or nitrous air. Some cases however have occurred where this evidently happens; and they appear so new and extraordinary, that he cannot but think they deserve the at-

tention of philosophical chemists. The history of the experiments alluded to is as follows.

2. As soon as he had heard of the production of inflammable air by the transmission of steam through red-hot iron tubes, he had the curiosity to try whether some other substances in the form of air or vapour might not, by a similar process, undergo material alterations. In particular, the nitrous acid seemed well to deserve a trial, both on account of the obscurity and difficulties attending the theory of its production, and also of its important and extensive usefulness in chemistry.

3. I began with boiling a little strong nitrous acid in a small retort, the neck of which was closely luted to one end of a gun-barrel. The other end of it was immersed sometimes in water, and sometimes in quicksilver, and 18 or 20 inches of the middle part was surrounded with burning charcoal in a proper furnace. In this manner the vapour and fumes of the boiling acid were transmitted through the red-hot tube, and the produce received at the end in the usual manner. When the acid was made to boil violently, there passed over a considerable quantity of undecomposed red nitrous vapour, together with a mixture of nitrous and phlogisticated airs. When the process was conducted more moderately, there was less nitrous vapour; and in the mixture of airs which was received in the glass vessels, there was a much greater proportion of phlogisticated air.

4. In order to increase the surface of the red-hot iron, and effect a more complete decomposition of the nitrous vapour, the gun-barrel was crammed full of iron filings. The experiments were repeated with great caution, and almost the whole of the produce was found to be phlogisticated air. It is however proper to mention, that notwithstanding every possible care, still there will generally be in some degree an admixture of nitrous air, and frequently of dephlogisticated nitrous air. But he is satisfied that if the iron tube were sufficiently long, so that a very large portion of it might be heated red-hot, all the air received in this manner from any quantity of nitrous acid slowly boiled, would be found of that species called phlogisticated air.

5. These experiments seem altogether analogous to those of Dr. Priestley, in which nitrous air, by exposure to iron, is converted first into dephlogisticated nitrous air, and afterwards into phlogisticated air. The only difference seems to be, that in these experiments the effect is brought about suddenly; whereas in the method of exposition to iron much time is required. And further, in this method of operating, it is very difficult to conduct the process so as to insure the production of that singular species of air called dephlogisticated nitrous air. If the acid boil very quick, the product is nearly all nitrous vapour and nitrous air. If it boil very slow, and a sufficient quantity of the iron tube be well

heated, then the decomposition is almost complete, and little is received but phlogisticated air. In both cases, the progress of the conversion of nitrous acid to the state of phlogisticated air seems to be the same. First, nitrous air is formed, then dephlogisticated nitrous air, and lastly phlogisticated air. This seems to be the natural order of the conversion. From what has been said, the most common process will probably appear to be, that a particle of the acid in the form of vapour first generates nitrous air; that the parts of this are applied to fresh surfaces of hot iron, and suddenly changed into dephlogisticated nitrous air; which, lastly, is applied to still fresh surfaces of the tube or fragments of iron, and so converted into phlogisticated air. When these successive contacts with fresh surfaces of hot iron are not sufficiently numerous or exact, it is not unnatural to conclude, that some portion of air may escape not perfectly decomposed.

6. These considerations induced Mr. M. to alter the process a little. Instead of boiling the acid in the retort, he put some thin pieces of copper into a phial, poured nitrous acid on them, and forced the nitrous air, as it was generated, to pass through the red-hot tube. The event answered his expectation; the decomposition was effected in this way easier than in the former. But before making this experiment, he examined what would be the effect of mere heat on nitrous air, as he had already learned from the experiments of others, that nitrous acid, forced in the form of steam through red-hot tubes of clay or glass, underwent the most important alterations. What might be the effect of long continued exposure to a red heat he could not say; but he was soon convinced, that nitrous air might be forced through a red-hot glass tube, without suffering any material change.

7. Lastly, he determined to try the effect of the gun-barrel on dephlogisticated nitrous air. For this purpose, he diluted a saturated solution of copper in the nitrous acid, and put pieces of iron wire into it, and as the neck of the retort which contained the solution was luted to one end of the gun-barrel, the dephlogisticated nitrous air was exposed in its passage to the action of the red-hot tube, and also to the surfaces of the red-hot iron turnings which it contained. In this case, when the process is conducted with proper care, all the air which is received at the other end of the tube will be found phlogisticated.

8. When the air received at the end of the gun-barrel was in the last mentioned state, viz. perfectly phlogisticated, Mr. M. frequently observed a white fume issuing along with the air, and sometimes ascending through the water or mercury into the glass receivers. On examining this white fume, he soon perceived by the smell that it contained volatile alkali.

9. Most of the experiments hitherto related were made in the summer of 1786; in general they agree with those of Dr. Priestley; the changes and pro-

ductions are much the same, and the only new circumstance is, as was observed at art. 5. The same effects are brought about instantly by the action of red-hot iron, which require much time by the method of simple exposure to cold iron. For which reason, though it gave him much pleasure at the time to see such curious transmutations brought about in a few minutes, yet it scarcely appeared worth while to trouble the R. S. with a detail of the experiments; and he only does it now, because the conjectures he then formed have been sufficiently verified by future experiments. The conjectures were as follow:

10. Almost immediately on seeing the volatile alkali produced by means of nitrous acid and metals, Mr. M. conceived the possibility of inverting the order of the process, and of producing nitrous acid or nitrous air by the decomposition of volatile alkali. He knew of no experiments where this had been done, or any thing like it; yet as volatile alkali was beyond all dispute produced in the method just described, and as the iron turnings and inside of the gun-barrel were left after the operation in a state of calcination, it seemed not unnatural to suppose, that by forcing volatile alkali through the red-hot calces of some of the metals, nitrous acid or nitrous air might be produced; though in fact he neglected for near 2 years actually to make the trial. It was some time in the month of March, 1788, that the calx of manganese on account of its very great infusibility, and its yielding abundance of dephlogisticated air, occurred as a very proper substance for the purpose. He immediately crammed a gun-barrel full of powdered manganese; and to one end of the tube he applied a small retort, containing the caustic volatile alkali. As soon as the manganese was heated red-hot, a lighted candle was placed under the retort, and the vapour of the boiling volatile alkali forced through the gun-barrel. Symptoms of nitrous fumes and of nitrous air soon discovered themselves, and by a little perseverance he was enabled to collect considerable quantities of air, which on trial proved highly nitrous. He afterwards frequently repeated this experiment, and always in some degree succeeded. Much depends on the kind of manganese employed, much on the heat of the furnace, and much on the patience of the operator; as these are varied, there will be great variations of the products.

11. In general Mr. M. made use of clean gun-barrels with which no previous experiments had been made. The manganese was used in rough powder; for when it is too finely powdered, the tube is choked, and the air cannot pass. In some experiments he applied the vapour of the volatile alkali directly to the hot manganese. In others he suffered the manganese to remain a considerable time in a red heat before he made the volatile alkali, contained in the retort at the end of the tube, to boil; and by this means informed himself of the nature of the airs which the manganese yielded per se. In neither case could he ever perceive the least appearance of nitrous acid or nitrous air till the volatile alkali was used.

Manganese, per se, gives airs of different kinds, but chiefly fixed and dephlogisticated airs, as soon as ever it is subjected to a considerable heat; but nothing nitrous comes from it, either on the first application of heat, or after it has been continued a long time; and he examined this point with great diligence. But soon after the volatile alkali begins to be applied, the jars in which the air is received will frequently turn slightly red, and this redness will increase on admitting atmospherical air.

Here however there exists a cause of deception, against which the operator ought to be on his guard, lest he should conclude that no nitrous air is formed, when in reality there is a considerable quantity. The volatile alkali, notwithstanding every precaution, will frequently pass over in great quantities undecomposed. If the receivers are filled with water, a great part of this will indeed be presently absorbed; but still some portions of it will mix with the nitrous air formed by the process. On admitting the atmospherical air, the nitrous air is decomposed, and the red nitrous fumes instantly combine with the volatile alkali. The receivers are presently filled with white clouds of nitrous ammoniac; and in this manner a wrong conclusion may easily be drawn, from the want of the orange colour of the nitrous fumes. A considerable quantity of nitrous air may have been formed, and yet no orange colour appear, owing to this circumstance; and therefore it is easy to understand how a small quantity of nitrous air may be most effectually disguised by the same cause.

12. These observations are made chiefly for the sake of those who may wish to repeat these experiments. The main point to be established, is the actual formation of nitrous air by this method. And this truth he considers as proved beyond all controversy; for by continuing the process patiently, and applying repeatedly fresh portions of strong volatile alkali to the same manganese, kept constantly hot in the gun-barrel, he often collected large jars of air, which was proved to be highly nitrous by mixture with atmospherical or with dephlogisticated air.

13. It is not easy to say whether, in this process, dephlogisticated nitrous air, or even nitrous acid itself, be not sometimes immediately formed by the action of the volatile alkali on the manganese. Traces of the former, in some instances, seem to discover themselves. As to the latter, it is very certain, that fumes of the nitrous acid often circulate in the jars that receive the air. But possibly these fumes may arise from the decomposition of nitrous air, by means of the superfluous dephlogisticated air of the manganese. 14. The steam of boiling water was applied to red-hot manganese in a similar way; not the least nitrous appearance; but the fixed and dephlogisticated airs were generated much more plentifully than when the manganese was urged by mere heat. When these airs had been collected in large quantities, the volatile alkali was applied as before to the residuum of the manganese, and nitrous air soon appeared.

15. As manganese is known to produce a very extraordinary change on spirit of salt in a moderate heat, it seemed not improbable, that a still greater change might take place by working in this method. Accordingly Mr. M. forced the vapour of boiling spirit of salt to pass through red-hot manganese. This experiment did not answer expectation; the product was a mixture of fixed and inflammable air. But it deserves to be noticed, that even in this case, after the effect of the spirit of salt had been tried for a long time, a production of nitrous air on the application of volatile alkali to the same manganese soon took place.

16. As there are many other substances besides the calx of manganese, which are known, per se, to afford dephlogisticated air, or a mixture of this with fixed air, it was natural to conclude from analogy, that such substances on the application of volatile alkali would not fail to afford nitrous air. It is best however in these matters to trust as little as possible to conjectures, and to bring every opinion to the test of experiment. Manganese is so singular a substance, that it is perhaps hardly safe, from what happens in making trials with it, to infer in any instance of another calx of a metal a similarity of effect. Red lead however, is known to agree in such a variety of chemical effects with manganese, that it was difficult to believe that the volatile alkali properly applied to it would not yield nitrous acid or nitrous air; yet he hitherto in vain attempted to bring this about. The red lead indeed melts during the process, flows into the cooler parts of the tube, and often chokes the passage of the air; but in some trials a great deal of air had been collected before that happened, and without any symptom of a nitrous mixture. It seems difficult to explain the reason of the failure; perhaps with a better adapted apparatus, and more perseverance, either the production in question may be obtained, or the cause of the failure discovered.

17. With calcined green vitriol Mr. M. had much better success. The salt was calcined to whiteness, and put into a gun-barrel; and, after several trials of forcing the volatile alkali through the hot tube, he procured by the operation some ounces of strong nitrous air. 18. As calcined green vitriol, per se, in a strong heat yields dephlogisticated air, Mr. M. had now no doubt but that any substance which had this property might, by similar treatment, be made to afford nitrous air. But in this supposition he was entirely mistaken. The volatile alkali was applied to some calcined alum at the moment when it was yielding in a strong heat plenty of dephlogisticated air. The product was an astonishing quantity of inflammable air, mixed with hepatic air and actual sulphur. The residuum of the alum had a strong hepatic smell, and contained particles of perfectly formed sulphur.

19. It now only remains briefly to propose what occurred as the probable theory and explanation of the facts related. The ingredients which enter into

the composition of nitrous acid seem to be the 2 principles or elements of the atmosphere, viz. phlogisticated and dephlogisticated air. That this is the case, there seems little reason to doubt. Both the composition and decomposition of nitrous acid renders the supposition probable. For, 1. Nitrous air and dephlogisticated air by mixture produce nitrous acid; and nitrous acid, by mere heat, is converted into a mixture of phlogisticated and dephlogisticated airs. 2. Nitrous air, by the methods already related, is changed into phlogisticated air, and these methods seem to consist in abstracting from the nitrous air a quantity of dephlogisticated air. 3. When nitrous acid and nitre are produced in a natural way, the process is not well understood; but the presence of the atmosphere is known to be necessary. 4. Mr. Cavendish's experiment is decisive on this point. The union of the 2 airs in question is effected by means of the electric spark, and nitrous acid is the product.

In the next place we are to consider, that volatile alkali contains phlogisticated air; for, 1. Volatile alkali, by mere heat, or by the electric spark, is changed into a mixture of phlogisticated and inflammable air; and, 2. The residuum of volatile alkaline air, after the calces of lead have been revived in it, is phlogisticated air. Therefore, when volatile alkali, in the form of fume or air, is applied to red-hot manganese, or calcined green vitriol (substances which are then yielding dephlogisticated air,) with these facts in view, it seems not difficult to conceive, that one of the ingredients of the alkali, viz. phlogisticated air, should combine with dephlogisticated air, and form nitrous acid or nitrous air. If nitrous acid be formed, it will indeed in that heat, as has been observed, be instantly decomposed; but if the effect of the union be nitrous air, that will sustain the heat without decomposition. How it happens that nitrous air should be formed, and not nitrous acid, or what the reason is, that nitrous air can sustain a red heat without decomposition, when nitrous acid cannot, Mr. M. is unable to say; and it is better to acknowledge our ignorance than advance groundless conjectures. So much may be pronounced as certain, viz. that nitrous air contains less dephlogisticated air than nitrous acid; because it requires the addition of dephlogisticated air to become nitrous acid.

And, lastly, the experiment with the calcined alum proves, that, in order to produce nitrous air, it is not sufficient merely to apply volatile alkaline air to a substance which is actually yielding dephlogisticated air. Perhaps the presence of another substance is required, which has a strong attraction for phlogiston. Perhaps, in the experiments with the calces of manganese and of iron, the inflammable principle of the volatile alkali combines with the calces of the metals, and the phlogisticated air, the other component part unites with the dephlogisticated air; and if so, it seems not improbable to suppose, that when alum is made use of, the inflammable principle of the volatile alkali having little or no

attraction for clay, the basis of the alum, should combine with its acid and form sulphur. If this reasoning be true, then it follows, that the vitriolic acid has a stronger affinity to the inflammable principle than it has to phlogisticated air; and the process with the green vitriol and manganese is to be explained by the operation of a double affinity: the inflammable principle of the volatile alkali joins with the calx of iron, the basis of the vitriol, or with the manganese, and the phlogisticated air with the dephlogisticated air produced by the acid in the red heat. Those who chuse to reject the doctrine of phlogiston must make the necessary alterations in these expressions; but the reasoning will be much the same.

END OF THE SEVENTY-NINTH VOLUME OF THE ORIGINAL.

I. Discovery of a Sixth and Seventh Satellite of the Planet Saturn; with Remarks on the Construction of its Ring, its Atmosphere, its Rotation on an Axis, and its Spheroidical Figure. By Wm. Herschel, LL. D., F. R. S. Anno 1790, *vol. LXXX*, p. 1.

In a short postscript, added to Dr. H's last paper on Nebulæ, he announced the discovery of a 6th satellite of Saturn, and mentioned that he intended soon to communicate the particulars of its orbit and situation to the R. S. He now however presents an account of 2 new satellites instead of one, which he discovered by means of his large 40-foot telescope; and has called them the 6th and 7th, though their situation in the Saturnian system intitles them to the 1st and 2d place. This he did that in future we may not be liable to mistake, in referring to former observations or tables, where the 5 known satellites have been named according to the order they have hitherto been supposed to hold in the range of distance from the planet.

The planet Saturn is, perhaps, one of the most engaging objects that astronomy offers to our view. As such it drew Dr. H's attention so early as the year 1774; when, on the 17th of March, with a 5½-feet reflector, he saw its ring reduced to a very minute line, as represented in fig. 1, pl. 7. On the 3d of April, in the same year, he found the planet as it were stripped of its noble ornament, and dressed in the plain simplicity of Mars. See fig. 2. Dr. H. passes over the following year, in which, with a 7-feet reflector, he saw the ring gradually open, till it came to the appearance expressed in fig. 3. He observes, that the black disc, or belt, on the ring of Saturn, is not in the middle of its breadth; nor is the ring subdivided by many such lines, as has been represented in divers treatises of astronomy; but that there is one single, dark, considerably broad line, belt, or zone, on the ring, which he always permanently found in the place where the figure represents it. From his observations it appears, that the zone on the

northern plane of the ring is not, like the belts of Jupiter or those of Saturn, subject to variations of colour and figure; but is most probably owing to some permanent construction of the surface of the ring itself. That however, for instance, this black belt cannot be the shadow of a chain of mountains, may be gathered from its being visible all round on the ring; for at the ends of the ansæ there could be no shades visible, on account of the direction of the sun's illumination, which would be in the line of the chain; and the same argument will hold good against supposed caverns or concavities. It is also pretty evident, that this dark zone is contained between 2 concentric circles, as all the phænomena answer to the projection of such a zone. Thus, in fig. 4, which was taken May 11, 1780, the zone is continued all round the ring, with a gradual decrease of breadth towards the middle answering to the appearance of a narrow circular plane, projected into an ellipsis.

With regard to the nature of the ring, we may certainly affirm, that it is no less solid and substantial than the planet itself. The same reasons which prove to us the solidity of the one will be full as valid when applied to the other. Thus we see in fig. 3 and 4, the shadow of the body of Saturn on the ring, which in fig. 3 is eclipsed towards the north, on the following side, and in fig. 4 about the middle, according to the opposite situation of the sun. In the same manner we see the shadow of the ring cast on the planet, where in fig. 1 and 2 we find it on the equatorial part; and May 28, 1780, it was seen towards the south. If we deduce the quantity of matter, contained in the body, from the power by which the satellites are kept in their orbits, and the time of their revolution, it must be remembered, that the ring is included in the result. It is also in a very particular manner evident, that the ring exerts a considerable force on these revolving bodies, since we find them strongly affected with many irregularities in their motions, which we cannot properly ascribe to any other cause than the quantity of matter contained in the ring; at least we ought to allow it a proper share in the effect, as we do not deny but that the considerable equatorial elevation of Saturn must also join in it.

The light of the ring of Saturn is generally brighter than that of the planet: for instance, April 19, 1777, the southern part of the ring, which passed before the body, was seen very plainly brighter than the disk of Saturn, on which it was projected; and on the 27th of the same month, with a power of 410, the 7-feet reflector had hardly light onough for Saturn, when the ring was sufficiently bright. Again, March 11, 1780, he tried the powers of 222, 332, and 449, successively, and found the light of Saturn less intense than that of the ring; the colour of the body with the high powers turning to a kind of yellow, while that of the ring still remained white. The same result happened on June 25, 1781, with the power 460.

Dr. H. comes now to one of the most remarkable properties in the construction of the ring, which is its extreme thinness. The situation of Saturn, for some months past, has been particularly favourable for an investigation of this circumstance; and his experiments have been so complete, that there can remain no doubt on this head. When nearly in the plane of the ring, he repeatedly saw the 1st, the 2d, and the 3d satellites, nay even the 6th and 7th, pass before and behind the ring in such a manner that they served as excellent micrometers to estimate its thickness by. It may be proper to mention a few instances, especially as they will serve to solve some phenomena that have been remarked by other astronomers, without having been accounted for in any manner that could be admitted, consistently with other known facts. July 18, 1789, at $19^h\ 41^m\ 9^s$, sidereal time, the 1st satellite seemed to hang on the following arm, declining a little towards the north, and gradually advanced on it towards the body of Saturn; but the ring was not so thick as the lucid point. July 23, at $19^h\ 41^m\ 8^s$, the 2d satellite was a very little preceding the ring; but the ring appeared to be less than half the thickness of the satellite. July 27, at $20^h\ 15^m\ 12^s$, the 2d satellite was about the middle, on the following arm of the ring, and towards the south; and the 6th satellite on the farther end, towards the north; but the arm was thinner than either of them. August 29, at $22^h\ 12^m\ 25^s$, the 3d satellite was on the ring, near the end of the preceding arm; and the arm seemed not to be the 4th, at least not the 3d, part of the diameter of the satellite, which, in the situation it was, seemed to be less than 1 single second in diameter. At the same time the 7th satellite, at a little distance following the 3d, was seen in the shape of a bead on a thread, projecting on both sides of the same arm: henee we are sure, that the arm also appeared thinner than the 7th satellite, which is considerably smaller than the 6th, which again is a little less than the 1st satellite. August 31, at $20^h\ 48^m\ 26^s$, the preceding arm was loaded about the middle by the 3d satellite. October 15, at $0^h\ 48^m\ 44^s$, he saw the 6th satellite, without obstruction, about the middle of the preceding arm, though the ring was but barely visible with the 40-feet reflector, even while the planet was in the meridian; however, we were then a little inclined to the plane of the ring, and the 3d satellite, when it came near its conjunction with the 1st, was so situated that it must have partly covered the first a few minutes after the time it was lost behind the house. In all these observations the ring did not in the least interfere with the view of the satellites. October 16, Dr. H. followed the 6th and 7th satellites up to the very disk of the planet; and the ring, which was extremely faint, opposed no manner of obstruction to seeing them gradually approach the disk, where the 7th vanished at $21^h\ 46^m\ 44^s$, and the 6th at $22^h\ 36^m\ 44^s$.

Many other instances might be brought, if necessary. There is however some

considerable suspicion, that by a refraction through some very rare atmosphere on the 2 planes of the ring, the satellites might be lifted up and depressed, so as to become visible on both sides of the ring, even though the ring should be equal in thickness to the diameter of the smallest satellite, which may amount to 1000 miles. As for the argument of its incredible thinness, which some astronomers have brought from the short time of its being invisible, when the earth passes through its plane, we cannot set much value on them; for they must have supposed the edge of the ring, as they have also represented it in their figures, to be square; but there is the greatest reason to suppose it either spherical or spheroidical, in which case evidently the ring cannot disappear for any long time. Nay he ventures to say that the ring cannot possibly disappear on account of its thinness; since, either from the edge or the sides, even if it were square on the corners, it must always expose to our sight some part which is illuminated by the rays of the sun: and that this is plainly the case, we may conclude from its being visible in the telescopes during the time when others of less light had lost it, and when evidently we were turned towards the unenlightened side, so that we must either see the rounding part of the enlightened edge, or else the reflection of the light of Saturn on the side of the darkened ring, as we see the reflected light of the earth on the dark part of the new moon. Dr. H. will not however decide which of the 2 may be the case; especially as there are other very strong reasons to induce us to think that the edge of the ring is of such a nature as not to reflect much light.

Dr. H. cannot leave this subject without mentioning both his own former surmises, and those of several other astronomers, of a supposed roughness in the surface of the ring, or inequality in the planes and inclinations of its flat sides. They arose from seeing luminous parts on its extent, which were supposed to be projecting points, like the moon's mountains; or from seeing one arm brighter or longer than another; or even from seeing one arm when the other was invisible. He was, in the beginning of this season, inclined to the same opinion, till one of these supposed luminous points quitted the edge of the ring, and appeared to be a satellite. Now, as he had collected every inequality of this sort, it was easy enough for him afterwards to calculate all such surmises by the known periodical time of the several satellites; and he always found that such appearances were owing to some of these satellites which were either before or behind the ring. Oct. 20th, for instance, at $22^h\ 35^m\ 46^s$, he saw 4 of Saturn's satellites all in one row, and at almost an equal distance from each other, on the following side; and yet the 1st satellite, which was the farthest of them all, was only about half-way towards its greatest elongation from the body of Saturn, as may be seen in fig. 5. How easily, with an inferior telescope, this might have been taken for one of the arms of Saturn, he leaves those to guess who know

what a degree of accuracy it must require to distinguish objects that are so minute, and at the same time so faint, on account of their nearness to the disc of the planet. On the whole therefore, he had not any one instance that could induce him to believe the ring was not of an uniform thickness; that is, equally thick at equal distances from the centre, and of an equal diameter throughout the whole of its construction. The idea of protuberant points on the ring of Saturn indeed is of itself sufficient to render the opinion of their existence inadmissible, when we consider the enormous size such points ought to be of, for us to see them at the distance we are from the planet.

From these supposed luminous points however, Dr. H. was, by imperceptible steps, brought to the discovery of 2 satellites of Saturn, which had escaped unnoticed, on account of their little distance from the planet, and faintness; which latter is partly to be ascribed to their smallness, and partly to being so near the light of the ring and disc of Saturn. Strong suspicions of the existence of a 6th satellite he had long entertained; and if he had been more at leisure 2 years before, when the discovery of the 2 Georgian satellites took him off the pursuit, he would certainly have been able to announce its existence as early as the 19th of August, 1787, when at $22^h\ 18^m\ 56^s$, he saw, and marked it down, as being probably a 6th satellite, which was then about 12° past its greatest preceding elongation.

In 1788 very little could be done towards a discovery, as his 20 feet speculum was so much tarnished by zenith sweeps, in which it had been more than usually exposed to falling dews, that he could hardly see the Georgian satellites. In hopes of great success with the 40 feet speculum, Dr. H. deferred the attack on Saturn till that should be finished; and having taken an early opportunity of directing it to Saturn, the very first moment he saw the planet, which was the 28th of last August, he was presented with a view of 6 of its satellites, in such a situation, and so bright, as rendered it impossible to mistake them, or not to see them. The retrograde motion of Saturn amounted to nearly $4\frac{1}{2}$ minutes per day, which made it very easy to ascertain whether the stars he took to be satellites really were so; and, in about 2 hours and a half, he had the pleasure of finding that the planet had visibly carried them all away from their places. He continued his observations constantly, whenever the weather would permit; and the great light of the 40 feet speculum was now of so much use, that he also, on the 17th of September, detected the 7th satellite, when it was at its greatest preceding elongation.

As soon as Dr. H. had observations enough to make tables of the motion of these new satellites, he calculated their place backwards, and soon found that many suspicions of these satellites, in the shape of protuberant points on the

arms, were confirmed, and served to correct the tables, so as to render them more perfect. Fig. 6 represents the 7 satellites of Saturn, as they were situated October 18, at $21^h\ 22^m\ 45^s$. The small star s served to show the motion of the planet in a striking manner; as, in about 3¼ hours after the above-mentioned time, the whole Saturnian system was completely moved away, so as to leave the star s as much following the 2d and 1st satellites, which then were in conjunction, as it now was before the 2d.

By comparing together many observations of the 6th satellite, Dr. H. finds that it completes a sidereal revolution about Saturn in $1^d\ 8^h\ 53^m\ 9^s$. And if we suppose, with M. De La Lande, that the 4th is at the mean distance of 3′ from the centre of Saturn, and performs 1 revolution in $15^d\ 22^h\ 34^m\ 38^s$, we find the distance of the 6th, by Kepler's law, to be 35″.058. Its light is considerably strong, but not equal to that of the 1st satellite; for, on the 20th of October, at $19^h\ 56^m\ 46^s$, when these 2 satellites were placed as in fig. 7, the 1st, notwithstanding it was nearer the planet than the 6th, was still visibly brighter than the latter.

The most distant observations of the 7th satellite, being compared together, show that it makes one sidereal revolution in $22^h\ 40^m\ 46^s$: and, by the same data which served to ascertain the dimension of the orbit of the 6th, we have the distance of the 7th, from the centre of Saturn, no more than 27″.366. It is incomparably smaller than the 6th; and, even in my 40 feet reflector, appears no larger than a very small lucid point. The revolution of this satellite is not nearly so well ascertained as that of the former. The difficulty of having a number of observations is uncommonly great; for, on account of the smallness of its orbit, the satellite lies generally before and behind the planet and its ring, or at least so near them that, except in very fine weather, it cannot easily be seen well enough to take its place with accuracy. On the other hand, the greatest elongations allow so much latitude for mistaking its true situation, that it will require a considerable time to divide the errors that must arise from imperfect estimations. The orbits of these 2 satellites, as appears from many observations of them, are exactly in the plane of the ring, or at least deviate so little from it, that the difference cannot be perceived. It is true, there is a possibility that the line of their nodes may be in, or near, the present greatest elongation, in which case the orbits may have some small inclination; but as he has repeatedly seen them run along the very minute arms of the ring, even then the deviation cannot amount to more than perhaps 1 or 2 degrees; if, on the contrary, the nodes should be situated near the conjunction, this quantity would be so considerable that it could not have escaped his observation.

From the ring and satellites of Saturn we now turn our thoughts to the

planet, its belts, and its figure. April 9, 1775, Dr. H. observed a northern belt on Saturn, which was a little inclined to the line of the ring. May 1, 1776, there was another belt, inclined about 15° to the same line, but it was more to the south, and on the following side came up to the place in which the ring crosses the body. July 13, the belt was again depressed towards the north, almost touching the line where the ring passed behind the body. April 8, 1777, there were 2 fine belts, both a little inclined to the ring. In like manner Dr. H. sets down many other similar observations till near the end of the year 1780; and then he adds, it will not be necessary to continue the account of these belts up to the present time; but that he had constantly observed them, and found them generally in equatorial situations, though now and then they were otherwise.

We may draw 2 conclusions from what has been reported. The first, which relates to the changes in the appearance of the belts, is, that Saturn has probably a very considerable atmosphere, in which these changes take place; just as the alterations in the belts of Jupiter have been shown, with great probability, to be in his atmosphere. This has also been confirmed by other observations: thus, in occultations of Saturn's satellites, they seem to hang to the disc a long while before they would vanish. And though we ought to make some allowance for the encroachment of light, by which a satellite is seen to reach up to the disc sooner than it actually does, yet, without a considerable refraction, it could hardly be kept so long in view after the apparent contact. The time of hanging on the disc, in the 7th satellite, has actually amounted to 20 minutes. Now, as its quick motion during that interval carries it through an arch of near 6°, we find that this would denote a refraction of about 2′, provided the encroaching of light had no share in the effect. By an observation of the 6th satellite, the refraction of Saturn's atmosphere amounts to nearly the same quantity; for this satellite remained about 14 or 15 minutes longer in view than it should have done; and as it moves about 2¼ degrees in that time, and its orbit is larger than that of the 7th, the difference is inconsiderable. What has been said will suffice to show, that very probably Saturn has an atmosphere of a considerable density.

The next inference we may draw from the appearance of the belts on Saturn is, that this planet turns on an axis, which is perpendicular to the ring. The arrangement of the belts, during the course of 14 years that Dr. H. had observed them, has always followed the direction of the ring, which is what he calls being equatorial. Thus, as the ring opened, the belts began to advance towards the south; and to show an incurvature answering to the projection of an equatorial line, or to a parallel of the same. When the ring closed up, they returned towards the north; and are now, while the ring passes over the centre,

exactly ranging with the shadow of it on the body; generally one on each side, with a white belt close to it. The step from equatorial belts to a rotation on an axis is so easy, and, in the case of Jupiter, so well ascertained, that he hesitates not to take the same consequence for granted here. But, if there could remain a doubt, the observations of June 19, 20, and 21, 1780, where the same spot was seen in 3 different situations, would remove it completely.

There is another argument, of equal validity with the former, which Dr. H. now mentions. It is founded on the following observations, and will show that Saturn, like Jupiter, Mars, and the Earth, is flattened at the poles; and therefore ought to be supposed to turn on its axis. July 22, 1776, he thought Saturn was not exactly round. May 31, 1781, it appeared as if the body of Saturn was at least as much flattened as that of Jupiter. August 18, 1787, the body of Saturn is of unequal diameters, the equatorial one being the longest. Sept. 14, 1789, $23^h\ 36^m\ 32^s$, having reserved the examination of the 2 diameters of Saturn to the present as the most favourable time, he measured them with the 20 feet reflector, and a good parallel-wire micrometer.

Equatorial diameter.		Polar diameter.	
1st measure,	21″.94	1st measure,	20″.57
2d	23 .11	2d	20 .10
3d	21 .73	3d	21 .16
4th	22 .85	Mean	20 .61
Mean	22 .81		

By this it appears that Saturn is considerably flattened at the poles. And as the greatest measures were taken in the line of the ring and of the belts, we are assured that the axis of the planet is perpendicular to the plane of the ring; and that the equatorial diameter is to the polar, nearly as 11 to 10.

We may also infer the real diameter of Saturn from these measures, which are perhaps more to be depended on than any that have hitherto been given. But as in his journal Dr. H. had measures that were repeatedly taken 10 years past, not only of the diameter of Saturn, but of the ring, and its opening, by which its inclination may be known; as well as of the distance of the 4th and 5th, and other satellites, which will be of great use in ascertaining the quantity of matter contained in the planet, he reserves a full investigation of these things for another opportunity.

One beautiful observation of the transit of the shadow of the 4th satellite over the disc of Saturn, he adds, to conclude this paper. Last night, November 2, 1789, at $23^h\ 13^m$ sidereal time, being always in quest of any appearance that may afford the means of ascertaining the rotation of Saturn on an axis, he discovered a black spot on the following margin of the disc of that planet. At $23^h\ 21^m$, he perceived a protuberance on the south preceding edge of the disc,

which he supposed to be the 4th satellite going to emerge. At 23^m, the black spot had advanced a little towards the preceding side. At 30^m, it was still advancing, and he saw that the spot was a little to the north of the equatorial belt, but so that a small part of it was on the belt. At 35^m, the black spot was a little more than $\frac{1}{4}$ of the diameter of Saturn advanced from the following edge towards the centre. At 39^m, the satellite was detached. At 49^m, the spot was advanced so as to be about $\frac{1}{3}$ of its way towards the centre; and the 4th satellite near half its own apparent diameter clear of the edge.

In this situation of the planet Dr. H. took an eye-draught of it (fig. 10) as it appeared with the black spot on the belt; the lately emerged 4th satellite; 2 parallel dark belts, the intermediate space between them and the equatorial one being a little brighter than the rest of the disc; the 6th, 3d, and 2d satellites on the preceding side; the ring projecting like 2 very slender lines on each side of the disc, and containing the 1st satellite on the following arm, with the 5th at a considerable distance following.

At $0^h\ 5^m$, the black spot was got a little more than half way towards the centre. It was much darker than the belt, and more on it than before. At $1^h\ 2^m$, by advancing gradually towards the south, it was now almost entirely drawn on the equatorial belt. At $1^h\ 13^m$, the black spot approached towards a central situation. At $1^h\ 21^m\ 51^s$, it was perfectly central, and at the same time on the middle of the equatorial belt. He followed the shadow of the satellite with great attention up to the centre, in order to secure a valuable epocha, which may serve to improve our tables of the mean motion of this satellite.

II. Astronomical Observations on the Planets Venus and Mars, made with a View to determine the Heliocentric Longitude and Annual Motion of the Nodes, and the Greatest Inclination of their Orbits. By Thos. Bugge, F. R. S., Regius Prof. of Astronomy at Copenhagen. p. 21.

1. *The heliocentric longitude and annual motion of Venus's nodes.*

The following astronomical observations were made at the Royal Observatory at Copenhagen with a 6-feet transit instrument, and with a mural quadrant of 6-feet radius. It is only necessary to set down the observed geocentric longitudes and latitudes, corrected for aberration and nutation, and compared with the tables of Dr. Halley and of M. de la Lande.

Mean time at Copenhagen.				Observed geocentric longitude of ♀.				Observed geocentric latitude of ♀.			Halley's error,		De la Lande's error,	
											in long.	in lat.	in long.	in lat.
	h.	m.	s.	s	°	′	″	°	′	″	″	″	″	″
1781, Sept. 13	1	38	6	6	18	39	15	0	28	12 N	+ 11	+ 3		
22	1	43	24	6	29	38	48	0	3	9	+ 17	+ 1	+ 39	+ 3
Oct. 1	1	49	46	7	10	36	42	0	23	48 S	+ 16	− 7	+ 44	− 2
4	1	52	11	7	14	15	17	0	33	3	+ 12	− 4		
1784, Sept. 20	0	37	17	6	9	40	27	1	6	15 N	+ 41	+ 15	+ 17	+ 12
25	0	39	57	6	15	52	59	0	57	50	+ 15	+ 3		
Oct. 2	0	44	49	6	24	35	34	0	44	21	+ 36	+ 6		
14	0	54	7	7	9	30	40	0	16	41	+ 10	+ 1		
21	1	0	42	7	18	13	11	0	1	3 S	+ 17	+ 1	+ 18	+ 3
1786, Aug. 19	2	25	57	6	4	45	54	0	23	7 N	− 5	− 3		
20	2	26	14	6	5	56	11	0	19	40	− 17	− 9		
21	2	26	32	6	7	6	42	0	16	20	− 13	− 5	+ 17	− 12
28	2	28	30	6	15	7	50	0	8	46 S	− 0	− 2	+ 23	+ 2
29	2	28	47	6	16	27	39	0	12	32	− 22	− 4		

The angle at the planet, or the commutation = P, is not directly to be taken out of the table. The difference between the observed geocentric longitude of the planet and the geocentric longitude of the sun, calculated from M. Mayer's solar tables, is the angle at the earth, or the elongation = T. From this elongation, which is to be depended on to a very few seconds, and from the planet's and the earth's distances from the sun, according to the tables, the commutation is calculated, and the geocentric longitude is reduced to the heliocentric longitude. The angles P, T, and S, at the sun, thus found, are likewise used to calculate the heliocentric latitude. According to the different dimensions, given to the orbit of the planet in the different tables, the radius vector at a given time will also be somewhat different. These differences in the tables of Dr. Halley and M. de la Lande are but small: thus, 1784, September 20, at $0^h\ 37'\ 17''$ mean time, the observed and apparent geocentric longitude of Venus $6^s\ 9^\circ\ 39'\ 54''$, the aberration and nutation $+ 33''$, the corrected and true geocentric longitude $6^s\ 9^\circ\ 40'\ 27''$, the sun's geocentric longitude $5^s\ 28^\circ\ 5'\ 51''$; hence the elongation $T = 0^s\ 11^\circ\ 34'\ 36''$. If now the logarithm of the radius vector SP be taken out of Halley's tables = 4.858251, then sin. $P = \frac{ST \times \sin. T}{SP}$, and $P = 16^\circ\ 11'\ 52''$; but if the logarithm SP be taken out of M. de la Lande's tables = 4.858168, the angle P will be found = $16^\circ\ 12'\ 4''$, the difference is $12''$. This uncertainty in the commutation, and consequently in the heliocentric longitude, would have been still greater if the calculations had been made only from the tables, or from the planet's geocentric longitude by the tables; thus this angle P is, according to Dr. Halley, = $16^\circ\ 10'\ 52''$; and from M. de la Lande = $16^\circ\ 10'\ 13''$.

Mean time at Copenhagen.				Heliocentric longitude of ♀ in the ecliptic.				Heliocentric latitude of ♀.			Halley's error,		De la Lande's error,	
	h.	m.	s.	s.	°	′	″	°	′	″	in long.	in lat.	in long	in lat.
1781, Sept. 13	1	38	6	7	28	40	26	0	56	13 N	+ 21″	+ 5″		
22	1	43	24	8	12	59	47	0	6	4	− 70	+ 2	+ 63″	− 6″
Oct. 1	1	49	46	8	27	16	26	0	44	6 S	+ 41	−13	+ 13	− 6
4	1	52	11	9	1	59	21	1	0	28	− 20	− 8		
1784, Sept. 20	0	37	17	6	25	51	19	2	23	46 N	+ 28	+34	+106	+27
25	0	39	57	7	3	52	40	2	13	7	+ 1	+ 7		
Oct. 2	0	44	49	7	15	6	32	1	40	46	+ 74	+13		
14	0	54	7	8	14	13	57	0	36	58	+ 33	+ 1		
21	1	0	42	8	15	21	40	0	2	17 S	+ 55	− 2	+ 43	+ 7
1786, Aug. 19	2	25	57	8	4	12	41	0	37	6 N	+ 25	− 4		
20	2	26	14	8	5	47	32	0	31	22	+ 52	−15		
21	2	26	32	8	7	23	10	0	25	55	+ 68	− 6	+ 69	−16
28	2	28	30	8	18	30	24	0	13	17 S	+108	− 3	+ 89	+ 3
29	2	28	47	8	20	5	16	0	18	52	+ 35	− 6		

Let the difference between two heliocentric longitudes, one before and the other after the passage through the node, be $= a$, the northern heliocentric latitude $= b$, and the southern $= \beta$; let the arc of the ecliptic from the node to the longitude, answering to the southern latitude, be $= x$; then tang. $x = \frac{\text{sin. } a \text{ . tang. } \beta}{\text{tang } b + \text{cos. } a \text{ tang. } \beta}$. By this formula the distance from every longitude with a southern latitude to the node may be found; and hence the heliocentric longitude of the node.

Observations compared.			Heliocentric longitude of ☋ ♀.			
			s	°	′	″
1781, Sept. 13	..	Oct. 1	8	14	42	42
13	..	4	8	14	42	23
22	..	1	8	14	42	29
22	..	4	8	14	42	12
		Mean	8	14	42	24
Reduced to 1786			8	14	44	53
1784, Oct. 21	..	Sept. 20	8	14	43	38
21	..	25	8	14	43	40
21	..	Oct. 2	8	14	43	42
21	..	4	8	14	43	30
		Mean	8	14	43	38
Reduced to 1786			8	14	44	34
1786, Aug. 19	..	Aug. 29	8	14	45	3
19	..	28	8	14	44	28
20	..	29	8	14	44	16
20	..	28	8	14	44	0
21	..	29	8	14	44	27
21	..	28	8	14	44	36
		Mean	8	14	44	28

Hence the heliocentric longitude of the descending node of the planet Venus was, 1786, August 25, at $8^h\ 39^m = 8^s\ 14°\ 44'\ 38''$, which is to be depended on to 10 or 15″. According to Cassini's tables, the longitude of the node is $8^s\ 14°\ 48'\ 31''$, the difference, or the error, − 3′ 53″. According to Halley's $8^s\ 14°\ 42'\ 39''$, the difference + 1′ 59″. From De la Lande's $8^s\ 14°\ 45'\ 15''$, the difference only − 37″.

In order to ascertain the annual motion of the node, this observation is to be compared with the observations of other astronomers. The numbers in the column A are found by setting out from my

own observations 1786. In the column B, M. de la Lande's observation of 1769 is taken as the first; the series C is begun with Mr. Horrox's determination of 1639; and in the series D, M. Cassini's observation 1698 is taken as the basis.

Astronomers' Names.	The time of observation.	Heliocentric longitude of ☊ ♀.				Annual motion of ☊ ♀.			
						A.	B.	C.	D.
		s	°	′	″	″	″	″	″
Horrox......	1639, Dec. 4	2	13	27	50	31.3	31.6	—	
Cassini......	1698, Sept. 4	2	14	1	45	29.2	29.2	36.5	
Cassini......	1705, June 11	2	14	2	52	30.9	31.3	31.9	
Cassini......	1731, April 7	2	14	17	2	30.0	30.2	32.7	24.9
De la Caille..	1746, Dec. 21	2	14	23	10	32.1	34.3	31.0	26.8
De la Caille..	1761, June 5	2	14	31	30	31.2	—	31.3	27.3
De la Lande..	1769, June 3	2	14	36	20	28.1	—	31.6	29.2
Bugge......	1786, Aug. 25	2	14	44	38	—	—	31.3	29.2
					Mean	30.4	31.3	32.3	27.5

If the mean be taken of these 4 means, the annual motion of Venus's node will be 30.37″, or nearly 31″, adopted in the tables of Halley and De la Lande.

2. *The greatest inclination of the orbit of Venus to the ecliptic.*

In the first place are set down the observed geocentric longitudes and latitudes, corrected for aberration and nutation.

Mean time at Copenhagen.				Geocentric longitude of ♀.				Geocentric latitude of ♀.			Halley's error,		De la Lande's error,	
											in long.	in lat	in long.	in lat.
	h.	m.	s.	s	°	′	″	°	′	″	″	″	″	″
1781, July 20	0	1	40	4	11	10	49	1	24	46 N	+ 30	+ 5		
24	1	5	45	4	16	5	52	1	27	21	+ 21	+ 5		
30	1	11	18	4	23	28	41	1	29	21	+ 29	+ 9	+ 69	+ 10
31	1	12	9	4	24	42	29	1	29	36	+ 32	+ 12		
Aug. 1	1	12	58	4	25	55	54	1	29	21	+ 18	+ 3		
4	1	15	21	4	29	37	13	1	29	4	+ 10	+ 7		
1782, July 13	21	9	19	2	10	54	17	2	12	22 S	+ 53	+ 2	+ 47	+ 3
Nov. 5	22	50	43	6	29	46	25	1	21	11 N	+ 40	+ 3	+ 65	+ 4
1783, Sept. 19	2	4	37	7	3	52	29	6	3	1 S	−115	+ 39	− 58	+ 24
20	2	2	2	7	4	15	51	6	10	26	−122	+ 49		
26	1	43	58	7	5	55	9	6	50	19	−190	+ 55		
Oct. 2	1	22	55	7	6	20	38	7	20	29	−130	+ 58	− 24	+ 44
1784, May 18	22	29	58	1	7	1	56	1	28	35 S	+ 57	+ 6	+ 44	+ 5
Sept. 8	0	30	11	5	24	45	11	1	20	2 N	+ 27	+ 3		
20	0	37	17	6	9	40	27	1	6	15	+ 41	+ 15	+ 77	+ 12
25	0	39	57	6	15	52	59	0	57	50	+ 15	+ 3		
1785, July 29	20	53	21	2	22	5	27	3	53	17 S	− 40	+ 13	− 13	+ 19
Nov. 27	21	29	57	7	9	30	40	1	37	43 N	− 5	+ 4	+ 26	− 2
1786, June 19	1	43	7	3	21	39	32	1	30	40 N	+ 12	+ 12		
24	1	49	20	3	27	44	41	1	36	52	+ 53	+ 19		
29	1	55	7	4	3	47	42	1	38	54	+ 3	+ 5		
July 1	1	57	17	4	6	13	9	1	39	45	+ 9	+ 7	+ 43	+ 8
14	2	9	7	4	21	54	24	1	37	40	− 6	+ 10		
1788, May 6	3	1	24	3	0	23	44	2	43	15 N			+ 20	+ 22
7	3	2	17	3	1	28	48	2	44	22			+ 27	+ 17
9	3	4	4	3	3	38	13	2	46	26			+ 39	+ 14

The angle at the planet is found in the manner before mentioned. The following table contains the heliocentric longitudes and latitudes to the moments of mean time in the foregoing table. The heliocentric place of the node is ascertained with a tolerable degree of accuracy; hence the arc of the ecliptic from the node to the circle of latitude, passing through the planet, is given $= d$; the inclination of the orbit to the ecliptic $= y$ is to be calculated by this formula, cot. $y = \frac{\text{sing. } d \times \text{sin. lat.}}{\text{tang. hel. lat.}}$.

	Heliocentric longitude of ♀ in the ecliptic.				Heliocentric latitude of ♀.				Halley's error, in long.	Halley's error, in lat.	De la Lande's error, in long.	De la Lande's error, in lat.	Inclination of ♀'s orbit.		
	s	°	′	″	°	′	″		″	″	″		°	′	″
1781, July 20	5	0	0	53	3	16	55	N	+ 18	+ 12			3	23	32
24	5	6	30	59	3	21	27		+ 7	+ 7			3	23	31
30	5	16	15	59	3	23	36		+ 12	+ 21	+115	+ 21	3	23	41
31	5	17	53	28	3	23	30		+ 16	+ 29			3	23	49
Aug. 1	5	19	30	31	3	22	43		+ 3	+ 7			3	23	27
4	5	24	22	35	3	20	41		+ 4	+ 18			3	23	35
1782, July 13	0	4	2	37	3	11	57	S	+116	+ 4			3	23	26
Nov. 5	6	9	46	4	3	4	18	N	+ 40	+ 5			3	23	27
1783, Sept. 19	11	6	41	21	3	21	45	S	− 97	+ 28	−183	+ 28	3	23	44
20	11	8	16	11	3	22	31		−105	+ 27			3	23	48
26	11	17	45	5	3	23	30		−175	+ 28			3	23	47
Oct. 2	11	27	16	44	3	18	56		−117	+ 31	−195	+ 30	3	23	49
1784, May 18	0	5	34	53	3	10	14	S	+ 71	+ 13	− 1	+ 11	3	23	34
Sept. 8	6	6	30	11	3	8	56	N	+ 18	+ 7			3	23	27
20	6	25	51	19	2	23	46		+ 28	+ 30	+116	+ 27	3	23	58
25	7	3	52	40	2	13	7		+ 1	+ 7			3	23	33
1785, July 29	11	15	44	9	3	23	30	S	− 68	+ 12	−154	+ 12	3	23	32
Nov. 27	6	0	50	45	3	15	22	N	+ 1	+ 2	+ 99	− 2	3	23	22
1786, June 19	4	25	53	12	3	12	43	N	+ 1	+ 26			3	23	41
24	5	4	2	2	3	20	31		+ 44	+ 43			3	24	0
29	5	12	8	55	3	23	18		− 5	+ 11			3	23	30
July 1	5	15	26	58	3	23	33		+ 1	+ 14	+109	+ 14	3	23	35
14	6	6	28	2	3	9	10		− 8	+ 18			3	23	38
1788, May 6	5	16	58	34	3	23	37	N			+ 20	+ 20	3	23	46
7	5	18	36	3	3	23	12				+ 27	+ 21	3	23	28
9	5	21	50	56	3	22	4				+ 39	+ 19	3	23	38
											Mean of all		3	23	37.7

The heliocentric latitudes observed 1781 July 30, 1783 Sept. 26, 1785 July 29, 1786 July 1, 1788 May 6, are very near the greatest latitude; the mean of the inclinations found on these days is 3° 23′ 40″.2, and very near the mean of all the observations 3° 23′ 37″.7. The inclination, or the greatest heliocentric latitude, may also be found by interpolation of the maximum among the observed heliocentric latitudes. This maximum is found 1781 = 3° 23′ 39″, 1783 = 3° 23′ 41″, 1786 = 3° 23′ 36″; the mean of these 3 maximums 3° 23′ 38″.6, which inclination may be depended on to 1 or 2″. The inclination of the orbit of

Venus has been supposed in the tables of Cassini, Halley, and De la Lande, = 3° 23′ 20″, and the error of the tables + 18″.6.

3. *The heliocentric longitude and motion of the nodes of Mars.*

In a Paper printed in the Memoirs of the Royal Academy of Sciences at Stockholm, is determined the heliocentric longitude of Mars's ascending node = 1ˢ 17° 54′ 24″.2, in the year 1783, December 7, 20ʰ 23ᵐ 39ˢ, mean time at Copenhagen: the error of Cassini's tables — 10′ 35″, of Halley's tables — 23′ 27″, of De la Lande's tables — 4′ 37″. The annual motion of Mars's node may be found by comparing the following observations of the longitude of the node. In the column A the numbers are going upwards from the observation 1783; in the column B the numbers are going downwards from the observation 1595.

Astronomers' Names.	Time of observation.	Heliocentric longitude of ☊ ♂.				Annual motion.	
						A.	B.
		s	°	′	″	″	″
Tycho Brahe ..	1595, Oct. 28	1	16	24	33	28.7	
Cassini	1700, May 6	1	17	13	43	28.9	29.4
Cassini	1721, Nov. 13	1	17	29	49	23.8	31.3
De la Caille ...	1747, May 14	1	17	37	11	27.5	28.9
De la Caille ...	1753, Nov. 4	1	17	42	5	24.6	29.6
Maskelyne	1778, April 17	1	17	51	40	28.5	29.3
Bugge	1783, Dec. 7	1	17	54	24		28.7
				Mean		27.0	29.5

The mean of the 2 series A and B will give the most probable annual motion of Mars's node 28″.2. In Cassini's tables the annual motion is 34″, in Halley's 38″, and in De la Lande's 40″.

4. *The inclination of the orbit of the planet Mars.*

Mean time at Copenhagen.				Geocentric longitude of ♂				Geocentric latitude of ♂.			Error of the tables of M. de la Lande, in long.	in lat.
	h.	m.	s.	s	°	′	″	°	′	″	″	″
1788, Jan. 9	11	57	7	3	16	27	7	4	5	25 N	+17	+ 1
10	11	51	28	3	16	3	35	4	6	10	+19	+ 7
11	11	45	50	3	15	40	15	4	6	44	+23	+10
26	10	24	50	3	10	43	39	4	3	59	+45	+19
Feb. 14	8	58	52	3	8	13	29	3	39	35	— 8	+ 8
Mar. 9	7	37	35	3	11	13	55	3	1	13	— 7	— 4
12	7	29	7	3	11	59	16	2	56	52	+ 2	+ 7
13	7	26	19	3	12	15	21	2	55	24	+ 2	+ 9
14	7	23	35	3	12	31	44	2	53	56	+ 3	+ 8
16	7	18	10	3	13	5	38	2	50	56	—18	+ 7
April 6	6	27	49	3	20	29	42	2	22	46.7	—17	+ 8

The geocentric longitudes of Mars are corrected for aberration and nutation, and compared with De la Lande's newest tables, which after the last improve-

ments commonly give the true place of Mars within the 4th part of a minute. The error in longitude + 17″ signifies that the longitude in the tables is 17″ too small; and that those 17″ are to be added to the calculated longitude, in order to make it agree with the observed longitude.

Mean time at Copenhagen.				Heliocentric longitude of ♂.				Heliocentric latitude of ♂.			Error of the tables of M. de la Lande, in long.	Error of the tables of M. de la Lande, in lat.	Inclination of the orbit of ♂.		
	h.	m.	s.	s	°	′	″	°	′	″	″	″	°	′	″
1788, Mar. 9	7	37	35	4	15	12	28	1	50	49.1 N	− 7	− 3	1	50	56
12	7	29	7	4	16	31	42	1	50	53.5	+ 2	− 4.5	1	50	56
13	7	26	19	4	16	58	8	1	50	56.0	+ 2	− 3	1	50	56.4
14	7	23	35	4	17	24	28	1	50	57.7	+ 3	− 1.3	1	50	57.7
16	7	18	19	4	18	16	50	1	50	56.7	− 4	− 2.3	1	50	56.7

The inclination of Mars is taken in Cassini's tables 1° 50′ 54″, and in the tables of De la Lande and Halley 1° 51′ 0″.

Mr. B. concludes this paper with the opposition of Mars according to the foregoing observations. The opposition of Mars to the sun happened 1788, Jan. 7, at 8^h 19^m 32^s true time; the apparent geocentric longitude of Mars at that moment = 3^s 17° 17′ 8″, and the geocentric latitude = 4° 4′ 3″ N. Saturn was in opposition to the sun, Aug. 29, 20^h 51^m 11^s true time; the apparent longitude ♄ = 11^s 7° 31′ 34″, and latitude 1° 59′ 33″ S. The new planet was in opposition to the sun Jan. 18, 0^h 28^m 33^s true time, the longitude = 3^s 28° 10′ 7″, and latitude 0° 34′ 35″ N.

III. An Account of some luminous Arches. By Mr. Wm. Hey, of Leeds, F.R.S. p. 32.

While Mr. H. was at Buxton, in March 1774, about half past 8 he saw a luminous arch, which appeared very beautiful in the atmosphere. Its colour was white, inclining to yellow; its breadth in the crown apparently equal to that of the rainbow. As it approached the horizon, each leg of the arch became gradually broader. It was stationary while he viewed it, and free from any sensible coruscations. Its direction seemed to be from about the N.E. to the S.W. at least its eastern leg was inclined to the north, and its western to the south. Its crown, or most elevated part, was not far from the zenith. The evening was clear, and the stars appeared bright. It continued about half an hour after it was first observed by the company.

In October 1775, he saw a similar arch at Leeds, of the same colour, breadth, and position. It began to disappear in 5 or 6 minutes after he had discovered it, without changing its situation. The manner in which it vanished was quite irregular; large patches in different parts, and of different dimensions, ceasing to be luminous, till the whole had disappeared. The evening was rather cloudy.

In the evening of March 21, 1783, between 8 and 9 o'clock, Mr. H. observed something like a bright cloud in the eastern part of the hemisphere, and also a similar appearance in the opposite part of the heavens. These luminous parts, which appeared in the eastern and western parts of the horizon, were connected by an arch of a fainter light.

It reached the horizon in the W.S.W. point. In its course it passed about 12° to the south of the zenith. Its breadth was about 9 or 10°. It remained visible about 10 or 12 minutes after he first discovered it, and then vanished gradually and irregularly. He observed no coruscations, nor any motion in this arch. A few minutes after another, and still more beautiful, arch made its appearance. It arose a point or 2 nearer the N.E. than the former had done. Its southern edge passed up a little to the north of the tail of the Great Bear, which was then in a vertical position. Its northern edge appeared at first a little to the south of the polar star; but, during the continuance of the phenomenon, it gradually receded about 10° to the south. The arch descended about the W.N.W.; but neither the eastern nor western extremities reached the horizon; each of them ending in a point gradually formed a little above the horizon. This arch might be about 10 or 12° at its vertex. It continued visible for half an hour; and though he could not discover any coruscations, or quick motion, in any part, yet the different portions of it were perpetually varying in the density of their light, and the whole arch, or at least its vertex, made a slow and equable motion towards the south. Where the light was the most dense, the smaller stars were rendered invisible by the arch, but stars of the 2d magnitude were not totally eclipsed by it. This arch disappeared, as the former, by patches; the light gradually becoming less intense. The colour of both these arches was white. Before the latter arch had entirely disappeared, a small one, not quite so broad as the rainbow, arose from its eastern leg, and ascending in a curvilineal direction to the polar star, terminated there. Its light was more faint than that of the other 2 arches; and it continued visible about a quarter of an hour. The evening was very fine when he saw these beautiful phenomena; the stars were bright, and there was not a cloud to be seen except in the horizon. There was a steady light in the north, without the least coruscation, extending from the N.E. to N.W. The wind blew from the N.E.

March 26, about the same time in the evening, Mr. H. was entertained with a similar appearance. He first observed 2 or 3 columns of aurora borealis shooting upwards in the north; and in a short time after a complete arch, like those already described, though somewhat different in its position. It arose between the E. and N. and N.E. points, passed obliquely to the south below Arcturus, and descended in the west through Orion, having almost the same direction through that constellation which the equator has. Its light was the most

faint about the vertex of the arch. Its most dense parts were continually varying in the intensity of their light. The larger stars were visible through its densest parts. It varied its position, and it continued visible about half an hour; but there was nothing which could be called a shooting or quick coruscation. There was a steady northern light all the evening, or at least till the arch had disappeared.

The grandest specimen of this phenomenon which Mr. H. had seen appeared on the 12th of April, between 9 and 10 in the evening. He perceived a broad arch of a bright pale yellow, arising between Arcturus and Lyra, about the right leg of Hercules, and passing considerably to the south of the zenith, its northern border being a little south of Pollux, and descending to the horizon near Orion, which was then setting. This arch seemed to be about 15° in breadth, and was of such a varied density, that it appeared to consist of small columns of light, which had a sensible motion. After above 10 minutes he saw innumerable bright coruscations, shooting out at right angles from its northern edge, which was concave, and elongating themselves more and more till they had nearly reached the northern horizon. As they descended, their extremities were tipped with an elegant crimson, such as is produced by the electric spark in an exhausted tube. After some time this aurora borealis ceased from shooting, and formed a range of beautiful yellow clouds, extending horizontally about a quarter of a circle. The greatest part of the aurora borealis which darted from this arch towards the north, as well as the cloud-like and more stationary aurora, were so dense, that they hid the stars from view. The moon was 11 days old, and shone bright during this scene, but did not eclipse the brightness of these coruscations. The wind was at north, or a little inclined to the east.

The last phenomenon of this kind which Mr. H. saw was on the 26th of April. About a quarter before 10 in the evening, he observed in the W. a luminous appearance, of the colour of the most common aurora borealis. From this mass or broad column of light issued 3 luminous arches, each of which made a different angle with the horizon. That nearest to the south seemed to arise at right angles with the horizon; while that nearest to the north made the smallest angle, and passed towards the N. E. through the constellation Auriga, having Capella close to its upper edge. He had not viewed them many minutes when they were rendered invisible by a general blaze of aurora borealis, which possessed the space just before occupied by these arches. He was soon satisfied that where the aurora borealis was dense, it entirely hid from view the stars of the 2d magnitude. He observed this particularly with respect to the star β in the left shoulder of Auriga. But the coruscations were never so dense as to render Capella invisible. The wind was between the N. and N. E. this evening.

After comparing the phenomena above described with each other, and with

those observed by Mr. Cavallo, in London; by Mr. Swinton, at Oxford; by Dr. Huxham, at Plymouth; and by Mr. Sparshal, at Wells, in Norfolk; Mr. H. cannot entertain a doubt, that these arches had all the same origin; and that they ought to be considered as a species of that kind of meteor called aurora borealis.

IV. Extract of a Letter from the Rev. F. J. H. Wollaston, M. A., F. R. S. (dated Sydney College, Cambridge, February 24, 1784,) to the Rev. Francis Wollaston, LL. B., F. R. S., containing the Observation of a Luminous Arch. p. 43.

I send you an account of a remarkable stream of light which appeared last night, from about $9^h\ 5^m$ to $9^h\ 25^m$, extending entirely across the hemisphere from W. to E. It rose from the horizon, about 10° S. of W., near δ and γ Ceti; thence ascended in a straight line, inclining a little S. to δ and ϵ Tauri, where it made an angle with its former course, and proceeded nearly in a vertical circle over β Aurigæ, ϑ Ursæ Majoris, by Cor Caroli to Arcturus, setting in the horizon about 20° N. of E. The light was steady, not undulating like Aurora; and as it converged towards the horizon at each end, had much the appearance which I conceive the tail of a comet must make whose nucleus is just in the horizon. That was particularly the case at the W. end, where it was brightest, becoming gradually fainter towards the zenith; the E. part was nearly of the same brightness. The greatest breadth of the stream in the zenith was about equal to the distance between the pointers in Ursa Major. It disappeared gradually. When first I saw it, it did not incline so much towards the S. at its W. end as afterwards; but rose directly up from δ Ceti to the zenith. I remarked this, because I never saw a stream extend so steadily across the heavens. There was very little of Aurora in any other part of the sky; indeed what would not have been observed at all, had it not been for this stream.

V. Of a Luminous Arch. By the Rev. Mr. B. Hutchinson. p. 45.

Last night, (Monday, Feb. 23, 1784,) at 9 o'clock, a very uncommon aurora borealis appeared at Kimbolton. When Mr. H. saw it, it had formed a perfect, uniform semi-circle, of the apparent breadth of half a yard, reaching like the rainbow (which it entirely resembled, only that its colour was simple,) from the W. S. W. horizon to that of the E. N. E. Some of the brightest stars of the Bull only just could be seen through it. The whole hemisphere was without a cloud; the wind had gone down at west; a slight frost, after a warm thaw, was taking place; and there was no other aurora borealis in the heavens till this began to fade away, which then however arose a little, due N., but without any

streamers; the ring had no vibratory motion. Its zenith distance on the meridian south 11°. Kimbolton is 63 miles N. N. W. of London, latitude 52° 20′.

VI. On a Luminous Arch. By J. Franklin, Esq., of Blockley. p. 46.

Mr. F. states that on Feb. 23, 1786, he observed a very odd appearance in the heavens about a quarter before 9 that evening. He was much surprized to see a white light, broader than a rainbow, pass across the heavens from east to west. It was a bright white light, about 5° wide in the zenith, and gradually coming to a point both ways. The eastern point terminated between Arcturus and the bright star in the knee of Bootis. The western point came nearly to the star marked a in the Whale's mouth. The southern side of the light was about 5° above Castor, passing eastward above Berenice's hair, and westward near Aldebaran, and through the Hyades. He observed it till 9 o'clock. Aldebaran was south of it when he first saw it; but it passed, and got north, before 9 o'clock. At 5 minutes past 9, no more of it was to be seen. It gradually went off in a few minutes. The sky was very clear from clouds, and the stars shone bright.

VII. Of some Luminous Arches. By Edward Pigott, Esq. p. 47.

Being at Kensington on Feb. 23, Mr. P. saw, at 9 o'clock at night, a very singular, luminous arch in the sky, about 4° in breadth, resembling much a bright white cloud, drawn out in great length, or something like the uncoloured northern lights, without flashes, but seemingly of a more substantial texture; the stars appeared very bright through it; it probably had already existed some time. At about $9^h\ 7^m$ he noted its track thus: it was visible very near the horizon in the N. E., passing between Arcturus and η Bootis, almost covered the cluster of Coma Berenices, and β Geminorum, then passed to the south of Aldebaran, over the stars o, g, or π Orionis, where its light was fainter, and disappeared a few degrees lower. Though its first appearance was that of a beautiful regular arch, after a few minutes, its form had varied a little, and became rather twisted, so that β ♊ was sometimes to the north or south of its centre, without being uncovered. At $9\frac{1}{4}^h$ its light was much fainter, broader, and more crooked. At $9^h\ 40^m$ its length was decreased, extending only as far as the Gemini's feet. It also had moved to the south of the cluster in Berenice, and of β ♊, passing through Cancer. Its breadth at this time was considerably increased, perhaps to more than double what it was at first, and its brightness much faded. The southern side became flaky, having about half a dozen parts hanging down, not unlike the tails of comets, the north side remaining even; it seemed approaching towards its dissolution. The north horizon exhibited a faint aurora borealis. Among the phenomena of this kind recorded in the

Philos. Trans. there are 2 resembling so exactly the above, that they deserve the consideration of the learned; one was seen in 1734-5, the other in 1749.

Some years before also, Mr. P. observed a few others, very similar to that just described; he therefore adds a short account of them, viz. at Brussels, March 14, 1774, at about 7 o'clock in the evening, the sky being very clear, there appeared an arch resembling a bright white fog, about 8 or 9° broad, tolerably well defined; the brightness of the stars it covered was diminished. The phenomenon lasted about ¾ of an hour. The air was cold, but not frosty. Towards midnight an aurora borealis was seen in the north, which appeared something like the phenomenon just mentioned. Again, March 15, 1774, at about 7¼ o'clock in the evening, a column of light appeared in the north, something like that of yesterday: weather very fine. Also, at Louvain, 1775, April 19, $9^h\ 39^m$, at night, after a storm, he saw a bright white line of light 1 or 2° in breadth, extending from N. N. E. through N. to N. W. almost parallel to the horizon, and elevated about 9°. It was brighter in the centre, and stars of the 3d and 4th magnitude which it covered were much diminished in brightness; it sometimes rapidly vanished and re-appeared, and altogether lasted near half an hour. Lastly, at Wickhill in Gloucestershire, 1777, Feb. 26, at about 7^h at night, he saw a faint white tract of light, not unlike a foggy column, about 6 or 8° in breadth. It extended from the horizon W. by S. to E. by S. passing over the stars in Orion's feet, and a very little to the north of Sirius. It seemed to have no motion, or to alter in brightness. The air was rather foggy, with a few clouds and a little wind. At about 10 o'clock a slight aurora borealis appeared in the north with streaks, extending sometimes to the zenith.

These kinds of lights seem to differ from the common aurora borealis in several particulars: their light is more condensed; they assume the form of an arch or column, and appear either to the north or south of the zenith, though he thinks oftenest to the south.

VIII. Experiments on the Analysis of the Heavy Inflammable Air. By Wm. Austin, M.D., Fellow of the College of Physicians. p. 51.

In a paper read before the R. S. in the year 1788, Dr. A. suggested an idea, that the heavy inflammable air is a compound of the light inflammable and phlogisticated airs. At that time he had observed, that the heavy inflammable air, or at least fixed air, is formed on the decomposition of nitrous ammoniac by heating it in close vessels; and that this air is affected by the electrical shock, like other elastic fluids into whose composition the light inflammable air enters. The conclusion he then drew from those facts seems to be supported by several subsequent experiments, which he here lays before the R. S. Several elastic fluids containing the light inflammable air, as the hepatic and alkaline airs, being de-

composed by the electric spark, Dr. A. was induced to try it on the heavy inflammable air, as soon as he suspected that it contained the lighter air as a constituent part. This experiment immediately detected the light inflammable air; for such an expansion took place as could not arise from any other known substance. Thus the heavy inflammable air was sometimes expanded to twice its original volume; and yet not a 6th part of the whole was found to have undergone a decomposition: for instance, when $2\frac{3}{4}$ measures were expanded to 6, it appeared by experiment, that nearly $2\frac{1}{4}$ measures remained in their original state. After the inflammable air has been expanded to about double its original bulk, he does not find that it increases further by continuing the shocks.

From this partial decomposition of the heavy inflammable air we obtain a mixture of the 2 inflammable airs with phlogisticated air; that is, of the heavy inflammable air not decomposed, of the light inflammable air disengaged by the spark, and of phlogisticated air. How much of this phlogisticated air preexisted in the heavy inflammable air, and how much was disengaged during the operation, it is not easy to determine. Neither are we acquainted with any substance which will separate the 2 kinds of inflammable air by combining with the one and leaving the other; but we know that dephlogisticated air will combine, in certain proportions, with each of them, either mixed or separate; that with one of them it forms fixed air, with the other water. Therefore, by inflaming dephlogisticated air with a mixture of these 2 airs, and observing the quantity of dephlogisticated air consumed, and the quantity of fixed air produced, we discover the excess of dephlogisticated air consumed, above what is sufficient for the production of the fixed air; and may conclude, that this excess of dephlogisticated air has combined with light inflammable air. This conclusion is further confirmed by attending carefully to the contraction which takes place on inflaming these airs, which is much greater in proportion to the quantity of fixed air produced, when a mixture of the 2 inflammable airs is inflamed, than when the heavy inflammable air is burnt alone. It is well known, that in all experiments of this kind, what remains after the combustion of the airs mixed together in due proportion, and after the separation of the fixed air, is chiefly phlogisticated air. From a considerable number of experiments conducted with great care and attention to all these circumstances, the Dr. endeavoured to approximate to the quantities of the phlogisticated and light inflammable airs disengaged, when a given quantity of the heavy inflammable air was decomposed. But all that could be attained to, was only an approximation to truth. The quantity of air decomposed by this method was so small, and the separation of the different parts into which it was resolved was attended with such difficulties, that an accurate analysis of the heavy inflammable air can never be obtained in this manner.

Dr. A. therefore attempted to decompose the heavy inflammable air by means of sulphur, which readily unites with the light inflammable air in a condensed state, and with it forms hepatic air. Having introduced some sulphur into a retort, filled with heavy inflammable air, and applied a sufficient heat to melt and sublime it, a considerable quantity of hepatic air was formed. After this air was absorbed by water, he could not perceive that the remaining air differed from the heavy inflammable air before the operation. Sulphur mixed with powdered charcoal, on being heated, yields hepatic air in great abundance, almost the whole of which is absorbed by water. The small unabsorbed residue, which does not exceed 100th part of the bulk of the whole air, appears to be phlogisticated air.

In whatever manner the heavy inflammable air was decomposed, whether by passing the electrical spark through it, by melting sulphur in it, or by heating sulphur and charcoal together, an appearance constantly occurred, which seemed to indicate, that volatile alkali is formed, whenever the heavy inflammable air is decomposed. The circumstance is this: a small piece of paper, stained with any blue vegetable substance, is turned green by standing in the air during any of these processes; and this green is changed to red on the addition of an acid. The inflammable air had been very long exposed to water, and had no such effect on blue vegetable substances before the operation. The Dr. has concluded these analytic attempts with several observations on the formation of fixed air from some substances, which consist only of the light inflammable, phlogisticated, and dephlogisticated airs, and from others, in which these 3 airs are combined with such matters as cannot be suspected of having any place in the composition of fixed air. He then gives a detail of the experiments on which these observations are founded. After which he adds: notwithstanding the utmost attention, we are liable to a small error in each of these experiments; and there is consequently a small variation in the results; but yet they concur sufficiently to justify the following conclusions. 1. That the heavy inflammable air contains the light inflammable air in great abundance. He apprehends this light inflammable air was, before the application of the electrical spark, a constituent part of the heavy inflammable air; because, if it were contained in the heavier air not as a constituent part, what should hinder its being burnt when the heavy inflammable air is burnt? Can it be supposed, that the heavy inflammable air should contain the light inflammable air in circumstances of combustion, and that the light inflammable air should escape the fire? And if the lighter air be burnt, the same quantity of dephlogisticated air would be necessary to saturate it before as after its being electrified. But it is evident from the preceding experiments, that much more dephlogisticated air is necessary to saturate the air, after it has been expanded by the electrical shock, than before.

2. That no fixed air is formed during the separation of the lighter air from the heavy inflammable air. Here it should be observed, that if the constitution of the heavy inflammable air depended on the union of the light inflammable and fixed airs, as some have supposed, we should certainly discover the fixed air, when the other part was separated from it. Or, should it be conjectured, that the light inflammable air is separated from water suspended in the heavy inflammable air, in that case, would not fixed air be formed from the other constituent part of the water uniting with the heavy inflammable air, in consequence of the repeated electrical shocks?

3. That the electrical shock separated a substance from the heavy inflammable air, which has some leading characters of an alkali. When inflammable air is decomposed by sulphur, or when hepatic air is made from charcoal and sulphur, we have the same appearance of an alkali. That this is the volatile alkali is evident from its evaporation, when hepatic air is made from sulphur and charcoal.

4. That the heavy inflammable air, through which the spark has been repeatedly passed, when burnt with any proportion of dephlogisticated air, does not produce so much fixed air, as the same quantity of inflammable air not electrified. Hence it is evident, that a part of the air is actually decomposed by the spark. Hence also we may infer, that the decomposed air is not resolved into light inflammable air and charcoal, of which some chemists have supposed it to consist, because the charcoal would combine with dephlogisticated air after its separation from light inflammable air, and we should not have such a defect of fixed air.

5. That the residues, after inflaming the decomposed air, are generally greater than those from the air in its natural state, or than can be accounted for from the mixture of the heavy inflammable and dephlogisticated airs. This affords a strong presumption, that phlogisticated air is extricated from the decomposed heavy inflammable air in a separate state, besides what enters into the volatile alkali, which is formed at the same time. If light inflammable air only were disengaged during the decomposition, the residues would certainly not be greater after inflammation with a sufficient quantity of dephlogisticated air; on the contrary, if the inflammable air were increased in proportion in the mixture, the combustion would be more complete, and the residues less.

Having observed, that sulphur readily combines with light inflammable air, if presented to each other at the instant that the inflammable air is detached from other bodies, before its particles have receded from each other, and that hepatic air is generally formed in this manner, he introduced some sulphur and heavy inflammable air into a glass retort, first filled with, and inverted in quicksilver, and applied a sufficient heat to melt it. The heat was continued till the sulphur was sublimed. The melted sulphur soon acquired a dark, reddish co-

lour; as it sublimed, it became quite black, and every part of the retort was covered with a black crust. On the depending part of the retort, where the melted sulphur lodged, and where the heat was strongest, there remained a black mark, which could not be removed by a much greater heat than that by which the sulphur was sublimed. The bulk of the air was not materially altered by this operation. A little blue paper being thrown up to the air after the operation, became green. Water absorbed about $\frac{1}{4}$ of it, and acquired a strongly hepatic smell. The inflammable air was carefully washed, so as to separate from it all the hepatic air. He then mixed this inflammable air with dephlogisticated air, and inflamed them, expecting to find a greater quantity of phlogisticated air in the residue, than when the inflammable air was burnt, which had not been subjected to this process. But the difference of the residue does not exceed $\frac{1}{15}$ the quantity of air decomposed in this manner, if we may judge from experiment.

The analogy between the heavy inflammable air and charcoal is illustrated by the formation of hepatic air from charcoal and sulphur. These substances, heated in a small glass retort, yield hepatic air in great abundance. The blue vegetable colour is turned green by exposure to this air. After hepatic air had been generated for a long time from the same materials, without admitting any common air into the retort, 99 parts in 100 of the air which came over last were absorbed by water. The insoluble part appeared to be phlogisticated air. Thus sulphur and charcoal, heated in a glass retort, yield hepatic air, phlogisticated air, and volatile alkali, or a substance very analogous to it.

As far as the Dr. has been able to discover by experiments, the heavy inflammable air and charcoal consist of the same elements in different proportion. The application of heat to pure charcoal confirms this opinion; for the production of heavy inflammable air from charcoal, by mere heat, is constantly accompanied with a production also of phlogisticated air. He apprehends, that in these cases the charcoal is decomposed and resolved into these 2 parts. Whenever charcoal or any substance containing it, is decomposed by heat only, the phlogisticated and heavy inflammable airs are produced; and when the heat is intense, Dr. Higgins has observed, that the air produced from these substances becomes rarer; probably in consequence of a portion of the heavy inflammable air itself being resolved by heat into its constituent parts. Dr. A. would not lay much stress on the appearance of phlogisticated air from the compound forms of vegetable, animal, and bituminous substances, all of which yield phlogisticated air and volatile alkali in great abundance; yet when the more simple modifications of the heavy inflammable air, as charcoal, vinegar, and, if Dr. Priestley is not mistaken, fixed air, give out phlogisticated air, when decomposed in close vessels, he cannot but infer, that phlogisticated air is an essential part of that pe-

culiar substance which exists in all these states, whether that substance be called charcoal, or the gravitating matter of heavy inflammable air.

Hence it appears, that the phlogisticated and heavy inflammable airs combined, constitute charcoal; and that the mere application of heat always resolves charcoal into these 2 substances. But the heavy inflammable air is itself a compound of the lighter inflammable and phlogisticated airs. If phlogisticated air be combined with the heavy inflammable, or, which is the same thing, if light inflammable air be taken from it, charcoal is reproduced; therefore, when sulphur is melted in the heavy inflammable air, and hepatic air formed in it, the remaining parts of the heavy inflammable air return to the state of charcoal. And lastly, when sulphur is melted in contact with charcoal, the decomposition is complete; and the charcoal is resolved into its ultimate particles, the phlogisticated and light inflammable airs, with a small admixture of volatile alkali.

Thus far he had proceeded in the decomposition of the heavy inflammable air. The formation of this air, on many occasions, confirms what has been said concerning its analysis. In the resolution of compound bodies into their constituent parts, it may always be suspected, that the whole is not accounted for, that some part may have eluded observation, till the very parts we assign are put together, and the same compound is produced from them. The frequent production of fixed air, from substances generally not supposed to contain the heavy inflammable air, has lately given rise to a new system in chemistry. The author of this system has the merit of pointing out the appearance of fixed air in almost all phlogistic processes, in the combustion of various substances, in the reduction of metals, and in the decomposition of acids; phenomena which cannot otherwise be accounted for, than by showing that the specific matter of charcoal is a compound body; that its component parts are present in all these processes; and in some of them nothing else, if we except dephlogisticated air.

Dr. A. has already taken notice of the formation of fixed air from nitrous ammoniac, which is now well known to contain nothing but the phlogisticated, light inflammable, and dephlogisticated airs. This salt, heated in close vessels, yields dephlogisticated nitrous air in great abundance, mixed with a small proportion of fixed air. He has often repeated this experiment with nitrous ammoniac, which indicated no trace of fixed air either with lime water, or with acids, before its decomposition; but, when the salt was decomposed by heat, he always found lime-water rendered turbid by the generated air; and, on adding an acid to the turbid lime-water, has observed air-bubbles to be produced in it. When the 3 elementary airs are in a condensed state, and are set free from any combinations, they unite and form fixed air without the assistance of heat. Thus fixed air is generally produced when metals are dissolved in the nitrous acid. In these solutions, the component parts of nitrous acid and the light inflammable

air, being extricated at the same time, unite before they have acquired the aëriform state, and constitute fixed air.

Objects are often too common or too near for our observation. Phlogisticated air presents itself in the decomposition of so many bodies, that its appearance excites no inquiry; and it is not regarded as essential to the chemical constitution of the bodies which yield it, excepting in the instances of nitrous acid and volatile alkali, 2 substances of very small extent in the scale of natural bodies. The calces of metals are well known to contain phlogisticated air; yet the effect of this air on calcination in general, and how far the very different calces of the same metal are influenced in colour or other properties by the different proportions of phlogisticated air, has never been considered. Fixed air is often formed from the calces of metals, mixed with water, or with some other substance containing light inflammable air. Red precipitate mixed with iron filings yielded very pure fixed air. Brass dust mixed with red precipitate, likewise gave out fixed air, though in less quantity. Turbith mineral and iron filings, treated in the same manner, afforded much less fixed air than the red precipitate and iron filings. It is probable, that the turbith mineral contains less phlogisticated air, than the red precipitate. The fixed air in all these experiments was mixed with phlogisticated and dephlogisticated air. Mr. Kirwan found, that the simple calx of mercury with iron filings and water produced fixed air. The same author also observed, that iron calcined with nitrous acid gave out, on being heated, fixed air; and he found the production of this air renewed on the addition of water. Dr. Priestley obtained fixed air from iron converted into rust by exposure to nitrous air. In all these experiments the 3 elementary airs are present, and, being expelled by heat from the metals with which they were combined, unite with each other, and form fixed air. It is not material to the present argument, whether the light inflammable air be supposed to be furnished from water, or from the regulus of a metal: it is enough for our purpose, that none of the substances employed in these experiments, contain heavy inflammable air or charcoal, in sufficient quantity to account for the fixed air produced, as Dr. Priestley has justly observed.

The growth of plants affords a strong proof of the formation of charcoal from the substances which have been assigned. If we may believe experiments, water and air alone are necessary to this natural process; yet vegetation is the great source of charcoal or heavy inflammable air. This inquiry is still in its infancy; but from the best experiments that have been made it should seem, that plants grow best in phlogisticated air; that they take in phlogisticated air, and give out dephlogisticated air. These phenomena cannot be accounted for but by supposing, that water is decomposed by growing plants; that part of its dephlogisticated air is discharged into the atmosphere; and that the other con-

stituent part of water, with phlogisticated air, is taken into the growing substance. Thus the phlogisticated and light inflammable airs are brought together by the process of vegetation.

IX. On the Strata and Volcanic Appearances in the North of Ireland and Western Islands of Scotland. By Abraham Mills, Esq. p. 73.

At Moneymore, in Ireland, Mr. M. first perceived tumblers of lava; hence by Maghera, Garvagh, Coleraine, Portrush, and to Bush-Mills, lava is continually seen, either in solid masses, forming the basis of the vegetable soil, or else in tumblers dispersed over the surface. He employed 2 days in studying the various appearances at the Giant's Causeway, and regretted being obliged to quit it so hastily. So much has been already said on this spot, that he only remarks that the red ochry joints between the beds of rude lava, and the different heights at which the basalt pillars are seen, give probability to the conjecture, that the whole mass has been the produce of several successive eruptions. He embarked at Port Ballintrea, and after 12 hours sailing arrived at Ilay, where, inspecting the lead mines, it was impossible to avoid noticing the singular appearance of those masses which run in a kind of veins in various directions, and are called Whyn Dykes, which had in some places a basaltic appearance.

On returning from Ilay he landed at Portrush; and, in his way to Ballycastle, viewed the Giant's Causeway from the top of the cliffs, and was much struck with seeing below, in the 4th or eastern bay, a kind of Whyn Dyke, which ran into the sea towards the N. N. E. Examining the cliffs at Ballycastle, he found the horses, or faults, of which there are several between the coals, were veins of lava, resembling the Whyn Dykes of Ilay, standing vertically, intersecting the various strata of coal and freestone, and running into the sea. The largest of the veins or Whyn Dykes is near 12 feet in breadth, and ranges N. by E. and S. by W.

Returning to Dublin, through Clogh, Ballymena, Antrim, Glanevy, Moira, Banbridge, Loughbrickland, and to within a short distance of Newry, he constantly saw tumblers of lava, and in some places the fixed mass of lava, in which were fissures ranging N. E. and S. W. When Mr. M. reached home, his mind being strongly impressed with the similitude between the Ilay Whyn Dykes and those of Ballycastle, which take their rise in a country confessedly abounding with volcanic matter; that he might be enabled to form a better judgement of their substance when he should again visit Ilay, he repeatedly and attentively examined the Derbyshire and toad-stone in the neighbourhood of Buxton, and found it very like the specimens of the Whyn Dykes, which he had brought from Ilay.

Early in the last summer Mr. M. again visited Ireland, and having spent some

time at the mines in the county of Wicklow, he proceeded to Belfast; and a little to the northward of that town he discovered in a bank a body of marl, running N. E. and S. W. between red and white sand-stone, the whole included and surmounted by a kind of toad-stone and rude lava, with joints having no particular direction. At Belfast he embarked for Ilay; but the wind obliged them to tide along the Irish shore, which, after passing Carrickfergus, chiefly consists of stupendous basalt cliffs. Farther north the cliffs are divided into horizontal beds of considerable thickness, by the intervention of a red substance, similar in appearance to that at the Giant's Causeway; near the water's edge, and under the lava, the white lime-stone is frequently seen; and these appearances continue all the way to Red Bay. Four miles from Clogh, under a bed of white lime-stone, 40 feet thick, saw the upper part of a bed of gneiss. Sailing from hence, plainly saw that the high broken point, which forms the N. E. point of Cushendun Bay, is composed of lava, with some rude appearance of pillars near the top; while close to the water's edge, and at some little distance in the sea, were tumblers of an immense size.

Entering the sound of Iona, saw that the rude coast of Mull, and the less elevated shore of Iona, were composed of red granite. At the landing place in Iona is laminated horn-stone; and a quarter of a mile north from the ruins of the cathedral is a vein of coarse red granite, 2 feet wide, standing nearly vertical, and ranging with the horn-stone E. N. E. and W. S. W.; on the surface are tumblers of red granite, and some few of lava. About a mile N. W. from the cathedral, and near the shore, is a vein, 2 feet wide, containing feld-spath and white mica, ranging E. and W. between granite sides. Many of the rocks are tinged with iron, and there is some bog iron ore in the mosses. In the S. W. part of the island, is a body of white marble, veined with pale green. At the Cove, where it is said St. Columb landed, the cliffs are of red granite, and the shore is covered with great variety of pebbles of serpentine, basaltes, granite, quartz, and other substances. Rowed from Icolmkill through the Bull Sound, which runs between Nun's Island and the island of Mull; on both sides the cliffs are of red granite, ragged and broken, without any regular beds or fissures, and having no particular range or inclination. Hence steered for Ardlun Head, which forms the S. W. point of Loch Leven, where they contemplated the wonderful arrangement of the basalt columns.

Near this place is a deep glen, running N. N. E. to the sea. It is about 30 yards in length, and 20 in breadth. The strata are disposed in the following extraordinary manner. The uppermost is 10 yards of lava, with horizontal divisions and vertical joints, taking the form of rude pillars. Under this is an horizontal bed of a perfectly vitrified substance, which appears to have been a shale, and is from 1 to 2 inches in thickness. Beneath this, is about 3 yards of a

siliceous gravelly concrete; below which are horizontal beds of indurated marl, of various thicknesses, from 6 to 12 inches. The whole of these beds, taken together, are about 4 yards. Lastly, are 10 yards of rude lava, containing specks of quartz and mica unaltered, pieces apparently of granite, and some nodules of calcined chert. The whole is incumbent on regular basalt pillars, of various dimensions, from 18 to 6 inches diameter, varying in the number of their sides, some having 5, some 6, and others 7 sides. They are also as variously disposed; those on the western extremity of the glen being straight, and lying horizontally; while of those on the east side some are bare, and standing perpendicularly; and others, which are surmounted by the rude lava, are inclined and curved, as if they had taken that form in cooling from the pressure of the incumbent weight. See Tab. fig. 14, pl. 6. Many of the pillars are very full of bladder-holes; the articulations of the joints are close, though not so close as those of the Giant's Causeway; but, like those, their tops, where exposed, are either concave or convex.

At the extremity of the glen is an insulated rock, supported by basalt pillars (fig. 15,) which are somewhat curved and inclined. Incumbent on these are other pillars, lying nearly horizontal, and having a rude face of lava to the westward. At high-water this rock is inaccessible without a boat; but at low-water it may be easily got at, by stepping from one tumbler to another; and on the north side it is not difficult to climb to the top. The bottom of the glen is covered with large tumblers of lava the whole way down to the rock, and presents the rudest scene imaginable. Opposite Ardlun Head, on the north side of Loch Leven, is Ben Vawruch, a high promontory, with strata in horizontal beds; and the hill being of a circular figure gives it the appearance of having several terraces, with a kind of castle or cairn on the top. The columnar pillars at Ardlun are more or less regular for an extent of near a mile and a half; and all the projecting points of Loch Leven, as far as the eye could reach, appeared to be composed of lava.

Landed without difficulty on the eastern side of Staffa. The greatest extent of the island is about 1 mile from N. E. to S. W. and in one part not more than a quarter of a mile from S. E. to N. W. It is tolerably level, the shore every where steep, and the cliffs formed by basalt pillars or rude lava. On the south side, rising from a nearly horizontal bed of reddish stone, are beautiful basalt pillars of considerable height, and standing vertically; at a little distance are others inclined, and others which are curved, very similar to the ribs of a ship. There are 3 caverns amidst the basaltic pillars; one of them is now usually called Fingal's Cave; but the school-master at Icolmkill said that the Erse name for it is Fein, which signifies the melodious or echoing cave. On the northern part of the island, and at the cove where they landed, the cliffs are of coarse lava,

without any pillars. In some parts of the island the tops of the pillars are standing bare; in other parts the surface is formed by a rude argillaceous lava, full of bladder-holes, some empty, others replete with quartz crystals. Calcareous spar, pebbles of indurated clay and shoerl, detached pieces of zeolite, are frequently seen, and the vegetable soil is a decomposed lava. In some places are met with gravel containing pebbles of basaltes, of red granite, and of quartz, their angles worn off, and they were become round and smooth.

In a small bay, about 1 mile to the S. E. of Ardlun Head, Loch Lyne, under a bed of jointed lava, which has some resemblance of pillars, and just at high-water mark, is a bed of coal, exactly 12 inches thick, intermixed with shale or bituminous shistus, dipping S. E. towards the loch 1 yard in 3; there is not any intervening substance between the coal and superincumbent lava, which contains many bladder-holes. Beneath the coal is also lava without any intervening matter. About 20 yards to the N. W. the coal again appears in the cliff, but is not more than from 8 to 10 inches thick. Here are tumblers of various sizes, scattered on the shore. Among them are some resembling the Derbyshire toadstone; and a short distance inland, to the S. W., are rude masses of lava, standing up at day, not unlike the great whyn dykes of Ilay. In the Loch, and at some distance from the opposite shore, there stood, within the memory of man, an insulated pillar of coal, from which the country people were accustomed to procure a supply for smiths' use; but the quantities they carried away, and the continual washing of the sea, have now entirely removed it. The island of Lismore, in the sound of Mull, is entirely limestone, excepting where it is crossed by the whyn dykes. In the island of Ulva are pillars somewhat resembling those of Staffa, but of a paler colour.—Canna also is basaltic, and resembles Staffa.—The Dutchman's Cap has rude pillars.—Cairnborough the same. Dunvegan in the isle of Skye has basaltic pillars, similar to Staffa.—On the S. W. side of the isle of Egg is a curious cavern.

Again embarked for Ilay; but, it being calm, and the tide against us, were obliged to anchor; and landed on an island which forms the S. E. point of the sound of Iona, which is a bare rock of red granite, broken and jointed in every direction. The upper surface of the granite, even in the very highest part, is all convex, which seems to prove, that by some convulsion it has been thrown up from the bed of the ocean, which, by long washing over it, had previously worn down its substance at the edges of all its numerous joints. On the east side of the point, and on the west side of a little bay, where the granite cliffs are at least 15 yards perpendicular, discovered a whyn dyke, or vein of lava, about 2 feet wide, included in a vertical fissure ranging S. E. by E. and N. W. by W. About 6 yards to the westward of the lava vein, or whyn dyke, is an immense fissure in the granite, ranging N. by W. and S. by E. It is from 9 to 10

feet wide, and, by estimation, about 120 feet deep. At the northern extremity, near the top, 2 stones are suspended in a most extraordinary manner between the sides: the under one is fixed, and on that the other appears to lie loose. (See fig. 16). There is a large cavern in the western side of the fissure, and a corresponding fissure is seen on the opposite shore.

Having shown that whyn dykes, or in other words veins of lava, are found in the vicinity of columnar basaltes, which latter are now, by almost universal consent, acknowledged to be of volcanic origin; Mr. M. now proceeds to describe the whyn dykes of Ilay. Ilay, from the northern to the southern extremes, is about 30 miles in length, and in one part extends nearly as much in breadth from the eastern to the western shores. After a rather minute account of several parts of the island, Mr. M. continues: The whyn dykes are too singular in their formation to escape the eye of the naturalist who traverses this island: they are masses, or rather veins, generally of a dark brown, apparently basaltic, matter, not unfrequently containing bladder-holes; from 3, 4, and 6 feet, to 8 or more yards in breadth, running in various directions. In some places they are straight for a considerable length; in others, their course, though progressive, is inflected; and in some parts they rise between 3 and 4 feet above the surface, forming natural boundaries or dykes, standing vertically, and appearing to fill up the chasms formed at some remote period in the strata. This is instanced in several of these dykes, in different parts of the island. One in particular stands vertically, is many yards in height, projects from the cliffs to the north-westward, and in that direction runs many fathoms into the sea. It bears the buffeting of the waves of the Atlantic Ocean from the south-west, and seems to defy their rage, though its breadth, compared with its height and length, is very inconsiderable, being not more than 5 or 6 yards wide. It is of a dark granular substance, very similar to the whyn dyke near Freeport, excepting that the central part is softer and of a paler colour. The outer sides, which are each about 2 feet thick, are of a very dark colour, hard, contain some bladder-holes and specks of zeolite, are generally detached from the centre by very small joints, and the whole is divided by transverse joints into irregular polygons of various dimensions. If this stupendous object is viewed from the north, it has much the appearance of a lofty wall of human fabrication. A small distance more to the southward is the great cave, in the Erse dialect called Ea mawr. The entrance is near 23 yards wide, and from 6 to 8 yards high. After going in a little way the roof rises, and the cavern extends in breadth; but at about 150 yards from the entrance, all its dimensions are contracted, and it becomes so small as barely to admit further progress by crawling on hands and knees. There are some calcareous stalactites pendent from the roof; and in this cave, as well as several others, wherever the water pervades through the joints of the chert, it tinges the sides of a ferrugi-

nous hue. Some veins of lead ore are also mentioned, but reported to be not worth the working.

If it be admitted, Mr. M. says, that he is right in his opinion of the volcanic origin of these different substances, a large tract will then be added to that already proved by others to have been subject to the effects produced by subterraneous fire; which, as far as has hitherto been discovered with us, commences in the s. w. part of Derbyshire, and is again seen in Seathwaite, about 5 miles from Hawkshead, in the N. W. part of Lancashire, and appears, N. W. from thence, in the neighbourhood of Belfast in Ireland, and ranging through the northern part of that kingdom; it is perceived in several of the western islands of Scotland, extending as far north as the island of Lewis, which is the northernmost of the Hebrides, and crossing east from Ilay, which is the southernmost, by Tarbut, Dumbarton, Stirling, and Edinburgh to Dunbar. Some persons may consider, with astonishment, the extent of those veins and masses of lava which appear in the northern part of the British isles, where no crater is visible; while others, who have read Von Troil, and recollect that he says, "That lava is seldom found near the opening of a volcano, but rather tuff, or loose ashes and grit," may perhaps unite with Mr. M. in opinion with Mr. Whitehurst, "that the crater whence that melted matter flowed, together with an immense tract of land towards the north, have been absolutely sunk and swallowed into the earth, at some remote period of time, and became the bottom of the Atlantic Ocean. A period indeed much beyond the reach of any historical monument, or even of tradition itself."

The more readily to compare the specific gravities of the Ilay lavas, and other substances, mentioned in this paper, with those from other parts, a table of their several weights is here added.

Ardlun coal	1.284
Jet, according to Dr. Watson	1.236
	1.180
Cannel coal from Haig in Lancashire	1.275
Whyn dyke, fr. near M'Arthur's head, N° 1	2.863
Whyn dyke from Freeport, inside N° 2	2.881
The same from outside, N° 3	2.850
Whyn dyke from Gartness, N° 4	2.631
5	2.698
6	2.484
7	2.542
8	2.322
Whyn dyke from Gartness, N° 9	2.833
10	2.652
Basaltes from the Giant's Causeway	2.743
——— from Fairhead	2.950
——— from Ardlun	2.724
——— from Staffa	2.736
Vitrescent substance from Ardlun	2.800
Toadstone, from great rocks, Derbyshire, yellow grey	2.133
dark compact	2.634
ditto cellular	2.528
yellow grey from Bonsal	2.219

Fig. 14, pl. 6, is a view of the glen near Ardlun Head in Mull.

Fig. 15, a view of the insulated rock at the termination of the glen.

Fig. 16, a view of the great fissure, the cave, and the suspended stones, in the island of Mull. The fissure ranges N. and S. is about 10 feet wide and 40 yards deep; the sides and the suspended stones are granite.

X. On the Height of the Luminous Arch that was seen Feb. 23, 1784. By Henry Cavendish, Esq., F. R. S., and A. S. p. 101.

This arch was observed, at the same time, at Cambridge by Mr. Wollaston; at Kimbolton in Huntingdonshire, by the Rev. Mr. Hutchinson; and at Blockley near Campden in Gloucestershire, by Mr. Franklin; and is described in letters from those gentlemen read to the R. S. in December 1786. See p. 627, &c. of this volume. It has been remarked, that as the arches of the kind described in these papers have usually but a very slow motion, their height above the surface of the earth may readily be determined, provided they are observed about the same time, at places sufficiently distant; and they seem to be the only meteors of the aurora kind whose height we have any means of ascertaining. The places at which this phenomenon was seen are not so well suited for this purpose as might at first be expected from their distance, because they lie too much in the direction of the arch; they however seem sufficient to determine its height within certain limits, and perhaps are as well adapted for it as any observations we are likely to have of such phenomena.

The latitude of Cambridge is 52° 12′ 36″; that of Kimbolton is said by Mr. Hutchinson to be 52° 20′, and according to the survey of Huntingdonshire, published by Jefferies, is 52° 19′ 50″; so that we may suppose it to be 7 geographical miles north of Cambridge, and by the maps it seems to be about 18 such miles west of it; and Blockley is by the map 12 geographical miles south and 72 west of Cambridge. At Cambridge the observations of its track seem to have been made at about $9^h\ 15^m$ P. M. or 8^h sidereal time. At Kimbolton, allowing for the difference of meridians, they could hardly have been made more than 5^m sooner; and at Blockley they were most likely made nearly at the same times as at Cambridge.

At Blockley the arch passed about 7° south of the zenith; but it is unnecessary to determine this point with precision. At Kimbolton it was found by a quadrant to pase 11° to the south of it; and at Cambridge it was observed to pass through δ and ϵ Tauri, β Aurigæ, θ Ursæ majoris, Cor Caroli, and Arcturus. Now, if an arch was drawn through these stars, it must have appeared sensibly waved to the eye; whereas Mr. Wollaston did not take notice of any crookedness in this part of its course. It is most likely therefore, that the middle of the arch must have passed to the south of β Aurigæ, and to the north of θ Ursæ; and if a circle is drawn through δ Tauri, Arcturus, and a point 1° north of the zenith, it will differ but little from a great circle, and will agree as well with the positions of these stars as any regular line which can be drawn, and will pass $2\frac{1}{2}$° below β Aurigæ, and as much above θ Ursæ; which is not a greater difference from observation than may well have taken place, considering how much care

and acquaintance with the fixed stars are required to determine a path by them so nearly.

The direction of the arch here described, in that part near the zenith, is W. 18° S.; and if a line be drawn through Cambridge in this direction, Kimbolton is 12.8 geographical miles north of it; and therefore, as the arch appeared 12° more south at Kimbolton than at Cambridge, the height of the arch above the surface of the earth must be $61\frac{1}{4}$ geographical or 71 statute miles. If we suppose that the middle of the arch really passed through β Aurigæ, the height comes out 52 statute miles. On the whole the height could hardly be less than 52 miles, and is not likely to have much exceeded 71.

The common aurora borealis has been supposed, with great reason, to consist of parallel streams of light shooting upwards, which, by the laws of perspective, appear to converge towards a point; and when any of these streams are over our heads, they appear actually to come to a point, and form a corona. Hence, from analogy, it seems not unlikely, that these luminous arches may consist of parallel streams of light, disposed so as to form a long thin band, pretty broad in its upright direction, and stretched out horizontally to a great length one way, but thin in the opposite direction. If this is the case, they will appear narrow and well defined to an observer placed in the plane of the band; but to one placed at a little distance from it, they will appear broader, fainter, and less well defined; and when the observer is removed to a great distance from the plane, they will vanish, or appear only as an obscure ill-defined light in the sky.

There are two circumstances which rather confirm this conjecture: first, that though we have an account of another arch besides this* having been seen at great distances in the direction of the arch, we have none of any having been seen in places much distant from each other in the contrary direction; and 2dly, that most of them have passed near the zenith, whereas otherwise they ought frequently to appear in other situations; for if they appeared near the zenith to an observer in one latitude, they should appear in a very different situation in a latitude much different from that. Mr. C. however, does not offer this as a theory of which he is convinced; but only as an hypothesis which has some probability in it, in hopes that by encouraging people to attend to these arches, it may in time appear whether it is true or not. If it should hereafter be found, that these arches are never seen at places much distant from each other in a direction perpendicular to the arch, it would amount almost to a proof of the truth of the hypothesis; but if they ever are seen at the same time at such places, it would show that the hypothesis is not true. Supposing the hypothesis to be well founded, the height above determined will answer to the middle part of the band,

* That of Feb. 15, 1750. Phil. Trans. vol. 46, p. 472 and 647.—Orig.

provided the breadth of it was small in respect of its distance from the earth; but otherwise will be considerably below the middle. If the breadth of the band was equal to the distance of its lower edge from the earth, the height of the lower edge would be $\frac{3}{4}$ of that above found; and if the breadth was many times greater, would be half of it.

In the common aurora borealis, an arch is frequently seen low down in the northern part of the sky, forming part of a small circle. What this is owing to, he cannot pretend to say; but it is likely that it proceeds from streams of light which appear more condensed when seen in that direction than in any other, and consequently that the streams which form the arch to an observer in one place, are different from those which form it to one at a distant place, and consequently that no conclusion, as to its height, can be drawn from observations of it in different places. Attempts however have been made to determine the height of the aurora from such observations, and even from those of the Corona; though the latter method must surely be perfectly fallacious, and most likely the former is so too.

XI. Observations on Respiration. By the Rev. J. Priestley, LL.D., F.R.S. p. 106.

When Dr. P. wrote the observations on the subject of respiration, published in the Phil. Trans. vol. 66, p. 226, he supposed, that in this animal process there was simply an emission of phlogiston from the lungs. But the result of his late experiments on the mutual transmission of dephlogisticated air and of inflammable and nitrous air, through a moist bladder interposed between them, and likewise the opinions and observations of others, soon convinced him, that, besides the emission of phlogiston from the blood, dephlogisticated air, or the acidifying principle of it, is at the same time received into the blood. Still however there remained a doubt how much of the dephlogisticated air which we inhale enters the blood, because part of it is employed in forming the fixed air, which is the produce of respiration, by its uniting with the phlogiston discharged from the blood; for such he takes it for granted is the origin of that fixed air, since it is formed by the combination of the same principles in other, but exactly similar circumstances.

Dr. Goodwyn's very ingenious observations prove, that dephlogisticated air is consumed, as he properly terms it, in respiration; but, for any thing that he has noted, it may be wholly employed in forming the fixed air above-mentioned. He has proved indeed, that the application of dephlogisticated air to the outside of a vein will change the colour of the blood contained in it. But this might have been effected by the simple discharge of phlogiston from the blood, when it had an opportunity of uniting with the dephlogisticated air thus presented to it. He o es not however seem to suppose, that there is any phlogiston discharged from

the blood in the act of respiration, but only that dephlogisticated air enters into it. But that Dr. P.'s former supposition, as well as Dr. G.'s, is true, will appear, he presumes, from the experiments which he will presently recite.

As, in order to determine what proportion of the dephlogisticated air destroyed by respiration is employed in forming the fixed air which is the produce of it, it was necessary to ascertain as exactly as possible the proportion of dephlogisticated air and of phlogiston in the composition of fixed air, Dr. P. repeated with particular care experiments similar to those he had formerly made for that purpose. He heated charcoal of copper in 41 oz. measures of dephlogisticated air of the standard of 0.33, till it was reduced by washing in water to 8 oz. m. of the standard of 1.33. Again, he heated charcoal of copper in 40.5 oz. m. of dephlogisticated air of the standard of 0.34, till it was reduced to 6 oz. m. of the standard of 1.76. And in each of these cases there was a loss of 6 gr. of the charcoal of copper; so that there cannot be more than 6 gr. of phlogiston in 33 oz. m. of fixed air, and consequently that only a very little more than ¼ of the weight of fixed air is phlogiston. He also heated perfectly well burnt charcoal of wood in 60 oz. m. of common air, and found ⅕ of the remainder to be fixed air, and the residuum of the standard of 1.7. Lastly, he heated 8¼ gr. of perfect charcoal in 70 oz. m. of dephlogisticated air, of the standard of 0.46, when it still continued 70 oz. m.; but after washing in water it was reduced to 40 oz. m. of the standard of 0.6, and the charcoal then weighed 1¼ gr.; so that from this experiment with common charcoal, as well as from the preceding with charcoal of copper, it appears, that about ¼ of the weight of fixed air is phlogiston, and consequently that the other ¾ are dephlogisticated air.

Having done this, he proceeded to ascertain how much fixed air was actually formed by breathing a given quantity both of atmospherical and of dephlogisticated air, in order to determine whether any part of it remained to enter the blood, after forming this fixed air. For this purpose he breathed in 100 oz. m. of atmospherical air, of the standard of 1.02, till it was reduced to 71 oz. m. and by washing in water to 65 oz. m. of the standard of 1.45. When the computations are properly made, as directed in a former paper, it will appear, that before the process this air contained 67.4 oz. m. of phlogisticated air, and 32.6 oz. m. of dephlogisticated air; that after the process there remained 53.105 oz. m. of phlogisticated air, and 11.895 oz. m. of dephlogisticated air; and that there were only 6 oz. m. of fixed air produced; for the quantity absorbed during the process could only have been very inconsiderable. It will therefore be evident that, in this experiment, 20.7 oz. m. of dephlogisticated air, which would weigh 12.42 gr. disappeared; whereas all the fixed air that was found would only have weighed 4.4 gr., and ¼ of this being phlogiston, the dephlogisticated air that entered into it would have weighed only 3.3 gr.; consequently 9.12 gr. of it

must have entered the blood; which is 3 times as much as that which did not enter, but was employed in forming the fixed air in the lungs.

He breathed in 100 oz. m. of dephlogisticated air, of the standard of 1.0, till it was reduced to 58 oz. m., and by washing in water to 52 oz. m. of the standard of 1.75, with 2 equal quantities of nitrous air. The computations being made as before, it will appear, that before this process, this air contained 66 oz. m. of phlogisticated, and 34 oz. m. of dephlogisticated air; and that after the process there were 30 368 oz. m. of phlogisticated air, and 21.632 oz. m. of dephlogisticated air. In this case therefore, the dephlogisticated air that disappeared was 13.3 oz. m. weighing 7.8 gr. and the fixed air was 6 oz. m. weighing 4.4 gr.; so that here also about 3 times as much entered the blood as did not. These experiments he repeated many times, and though not with the same, yet always with similar, results, the greatest part of the dephlogisticated air, but never the whole, passing the membrane of the lungs, and entering the blood.

When the results above-mentioned are compared, it will appear, though the observation escaped Dr. Goodwyn, that part of the phlogisticated air entered the blood, as well as the dephlogisticated air; or, which is the same thing, that the dephlogisticated air which was consumed was not of the purest kind. This experiment Dr. P. repeated so often, and always with the same result, that he is confident he cannot be mistaken in this conclusion. This fact, of which he had no previous expectation, he first thought might be accounted for by supposing that the 2 constituent parts of atmospherical air, viz. the phlogisticated and dephlogisticated air, are not so loosely mixed as has been imagined; but rather that they have some principle of union, so that, though they may be completely separated by some chemical processes, they are not entirely so in this; but that the dephlogisticated air, passing the membrane of the lungs, carries along with it some part of the phlogisticated with which it was previously combined. But, at the suggestion of Dr. Blagden, he now thinks it more probable, that the deficiency of phlogisticated air was owing to the greater proportion of it in the lungs after the process, than before.

XII. An Account of the Trigonometrical Operation, by which the Distance between the Meridians of the Royal Observatories of Greenwich and Paris has been determined, By Major-general Wm. Roy, F. R. S., and A. S. p. 111.

The trigonometrical operation which is the subject of the present paper, had its commencement in the measurement of a base on Hounslow-heath in 1784, an account of which was given to the R. S. in the following year. On the completion of that first part of the business, it was little expected, that nearly 3 full years would have elapsed before an instrument could be obtained from Mr. Ramsden for taking the angles! At length however the instrument was pro-

duced, and placed on the 31st of July 1787, at the station near Hampton Poor-house, on the very spot where, about 35 months before, the measurement of the base had been completed. By commencing an operation of this nature, at so advanced a season of the year, it was sufficiently obvious, that only very faint hopes could be entertained of bringing it to a conclusion before the bad weather would set in. But it being of much importance to get the triangles, which extend across the Channel, at all events executed, it was therefore proposed to M. Cassini, who had been appointed by the Academy of Sciences to superintend their part of the business, that he should fix the time that might suit him best for meeting on the coast. This proposition being readily acceded to by him, the 20th of Sept. was appointed for repairing to the coasts of Dover and Calais respectively. In the mean time the operation was continued here with all imaginable care and assiduity, through the first 10 stations of the series of triangles from Hampton Poor-house to that at Wrotham-hill inclusively.

The instrument, and the various parts of the apparatus, were then removed to Dover, at which place Messrs. De Cassini, Mechain, and Le Gendre, members of the Academy of Sciences, arrived on the 23d of Sept., where, in the course of 2 days that these gentlemen staid, every thing was most amicably settled with regard to the times of reciprocal observations. A great number of white lights, fitted for long distances, and several reverberatory lamps, had been previously provided. Having been supplied with such a proportion of the lights as seemed necessary for their side of the channel, and one of the lamps, the French gentlemen departed for Calais on the 25th. For the greater part of the time, the weather was extremely bad; yet, on the particular nights when the most important observations on our side were made, namely, those at Dover and Fairlight Down, the nights happened very fortunately to be favourable, so as to enable us to intersect, with great accuracy, the two distant points on the French coast of Blancnez and Montlambert, or Boulemberg, and thereby to establish for ever, the triangular connection between the two countries. In finishing the co-operation with the French commissioners, at Lydd on the 17th of October, our instrument had now passed through 16 stations out of 23. There of course remained yet 7 stations where it was to be placed, and observations to be made. Eagerly wishing to bring the business to a conclusion, they struggled on through 5 of the 7. But the weather at length became so tempestuous, that it was utterly impossible to continue it, with any hopes of being able to make satisfactory observations. On the 2d of November therefore the instrument was sent to town, leaving the stations on Goudhurst and Frant Churches unoccupied till the ensuing season, and the winter months were employed in calculating the observations that had been made.

By various delays in repairing the instruments, the surveyors had again the

mortification to be thrown into the latter season of the year, as it could not be placed on Goudhurst steeple before the 9th of August 1788. Having finished the few remaining observations, the paper proceeds to state the manner in which the calculations are made, and the account drawn up in several sections, viz.

Sect. 1. Description of the apparatus made use of in the measurement of the base of verification in Romney Marsh, with the hundred-feet steel chain, in the autumn of 1787, with the result of that operation. 2. General description of the great instrument with which the angles in the recent trigonometrical operation were observed; showing also its various adjustments for practice. 3. Description of various articles of machinery made use of in the course of the trigonometrical operation. 4. Calculation of the series of triangles extending from Windsor to Dunkirk, by which the geodetical distance between the meridians of the Royal Observatories of Greenwich and Paris is determined. 5. On the difference between horizontal angles on a sphere and spheroid. 6. Manner of determining the latitudes of the stations. Application of the pole star observations to computations on different spheres, and also on M. Bouguer's spheroid, for the determination of the difference of longitude. Ultimate result of the trigonometrical operation, by which the difference of the meridians of the Royal Observatories of Greenwich and Paris is determined. 7. An account of the observations made during the course of the trigonometrical operation for the determination of terrestrial refraction. 8. Secondary triangles, subdivided into 2 sets, for the improvement of the maps of the country, and the plan of the city of London and its environs. And the conclusion, containing propositions for extending trigonometrical operations over Great Britain.

On several accounts it is not necessary to enter into the particulars of this extremely long and very detailed account of the measurements and calculations of this important and extensive survey; particularly as this, and the former years operations of this kind, have been collected into volumes, and published separately, by Mr. Faden at Charing Cross; and also as it is stated by the R. S. in an appendix to this paper, that very numerous and great errors have been committed in the calculations, &c. so as to render the recomputation and reprinting of many sheets unavoidably necessary. This circumstance is thus stated by Dr. Blagden, one of the secretaries of the R. S. in the appendix, at p. 591.

" Our late much respected colleague, Major-general Roy, having finished, in September 1788, the trigonometrical measurement described in the first part of this volume, returned to London in a very indifferent state of health. From this time he employed all the leisure that his illness, and his various official avocations, allowed, in preparing the account of his operations, to be laid before the R. S. But toward the autumn of 1789 his infirmities increased so much, that the medical gentlemen he consulted advised him to spend the following winter at Lisbon,

for which place he accordingly embarked in the beginning of November. Previous to this however he finished the first copy of his paper; but it was much hurried toward the latter part, and not rendered so perfect as the General would undoubtedly have made it with more time and better health. He returned to England in April 1790, and the paper was sent to the press before the end of the same month. Unfortunately the General did not live to see the printing quite completed; he corrected indeed all the sheets except the last 3; but without comparing his manuscript copy with the original papers and observations. Several errors which had been discovered in the course of the printing, together with the obscurity of the account in certain parts, induced some of the General's friends, members of the R. S., to request, after his decease, that the whole might be revised by a competent person, who should compare it with the original documents, correct such mistakes as might be discovered, and illustrate whatever required further explanation. No one could be found so proper for this task as Mr. Dalby, the gentleman of whom the General makes such honourable mention in his paper, and who, having assisted in all the operations, was as well acquainted with every part of them as the General himself. The result of Mr. Dalby's examination is the following remarks; which being much too long for insertion in the list of errata, where only the errors of the press are noticed, is here added separately, by way of appendix. C. BLAGDEN."

This task was accordingly very ably executed by Mr. Dalby, and his corrections printed, amounting to 22 quarto pages of the volume, under the title of Remarks on Major-General Roy's account of the Trigonometrical Operation, &c.

A Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council. p. 271.

A summary of the whole observations in the year 1789.

1789.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Inches.
	°	°	°	°	°	°	Inches.	Inches.	Inches.	
January	53.0	17.5	35.7	56	36	46	30.75	28.58	29.72	1.345
February ..	51.0	34	42.5	57	49	52.5	30.34	28.65	29.70	1.605
March	46.5	26	36.6	52	44	47.8	30.13	28.94	29.72	1.549
April	62	29	47.2	61	48	54.2	30.18	29.10	29.77	0.957
May	66	45	56.9	65.5	55	60.5	30.27	29.57	29.88	1.103
June	72	50	58.5	66	58.5	61.5	30.23	29.40	29.84	3.244
July	71	52	61.9	65	59	63.5	30.09	29.54	29.85	2.467
August	74.5	54	63.7	69	62	66.2	30.33	29.70	30.06	1.864
September..	74	45	57.3	68	58.5	62.6	30.38	29.30	29.88	2.155
October	59	36	49.1	63	53	57.5	30.29	29.00	29.52	3.253
November..	55	28.5	41.0	56	48	52.6	30.16	28.72	29.70	1.244
December ..	53	33	43.5	58	48	53.6	30.56	28.88	29.86	1.190
Whole year			49.5			56.5			29.79	21.976

XIII. An Account of the Tabasheer. By Patrick Russell, M. D., F. R. S.* p. 273.

This drug was first introduced to the knowledge of the western world through the works of the Arabian physicians, all of whom mention it as an important article in their Materia Medica; and, from what Dr. R. could observe in Syria, it still continues to be in much more general use in Turkey than in India. To the Arabs and Turks it is known under the name of Tabasheer only; under that name also it is mentioned by the Arabian writers. In this country, [Vizagapatam,] besides that of Tabasheer, which they had from the Persians, it is known under several other names. In the Gentoo language it is called Vedroo-Paloo, bamboo-milk; in the Malabar, Mungel Upoo, salt of bamboo; and in the Warriar, Vedroo Carpooram, bamboo camphor. Don Garzia dall' Horto has long ago exposed a dangerous error, common to the old translators of the Arabian writers, respecting this drug. In the Latin versions of Rhazis and Avicenna, Tabasheer is constantly rendered spodium; and this interpretation has been adopted by most of the subsequent translators of other Arabian medical writers.

The late Mr. Channing, when engaged in the translation of Rhazis on the small-pox, applied to Dr. R. then in Syria, for such information as he might be able to collect on the subject of Tabasheer at Aleppo. Dr. R. accordingly transmitted to him various specimens of the drug, together with several extracts relative to it, from books found in the Aleppo libraries. Some of those specimens differed considerably from these now laid before the R. S.; and from what he has had occasion to observe during his residence in India, he is convinced that much of the drug commonly vended in Turkey is fictitious or adulterated. He thinks the Arabian medical writers generally agree in the Tabasheer being a production of the Indian reed; more especially of such as have suffered from fire, kindled by the friction of the reeds one against the other; an accident supposed to happen frequently in the dry season, among the hills, where the bamboo forms vast and impenetrable thickets. Several of the mountaineers assured him that the bamboo is not the only tree subject to accidental ignition by friction, and named one or two other trees liable to the same accident; but added, they never looked for Tabasheer in the half-burnt fragments of the bamboo, though they doubted not it might sometimes be found there as well as in others. The genuine Tabasheer is doubtless a production of the Arundo Bambos of Linnæus, the Ily of the Hortus Malabaricus, and the Arundo Indica arborea maxima, cortice spinoso, of Herman. It is no less certain that fire is not a necessary

* Brother to Dr. Alexander Russell, (for a biographical notice of whom see Vol. X. p. 667 of these Abridgements,) and formerly physician to the British Factory at Aleppo. Dr. Patrick Russell was author of a Treatise on the Plague, (particularly valuable for the observations on quarantine, and for the regulations recommended to be adopted during the prevalence of a pestilential contagion,) and of a Natural History of Indian Serpents, illustrated by coloured engravings. He died in London in 1805.

agent in its production, whether the conflagrations in the mountains just now mentioned be reckoned fabulous or not. The bamboo in which the Tabasheer is found is vulgarly called the female bamboo, and is distinguished by the largeness of its cavity from the male, employed for spears or lances. They are said to be separate trees.

Of the 7 pieces of bamboo which accompany this paper, 4 are from the mountains in the vicinity of Vellore, and 3 from a place 20 miles from hence. The former were perfectly green on their arrival at Madras; and the others were selected from a large parcel, which were green also when they came to hand. These were all selected on a conjecture of their containing Tabasheer, from a certain rattling perceived on shaking the bamboo, as if small stones were contained in the cavity. This is considered by the natives as an indication of Tabasheer being contained in one or more joints of the bamboo, and they are seldom disappointed; but it does not always follow that there is no Tabasheer where a rattling is not perceptible; for, on splitting a number of reeds, it was sometimes remarked, that where the quantity of the drug was inconsiderable, it was found adhering so closely to the sides of the cavity, as to prevent any rattling from being perceived on shaking. In general however the rule of the natives for choosing the bamboos proved a good one.

In April, one of the bamboos, consisting of 6 joints, received from Vellore, being cautiously split, each joint was examined separately. In 2 of them no vestige of the drug was discovered; each of the others contained some, but in various quantity; the whole collected amounted to about 27 grs. The quality also was various. The particles reckoned of the first quality were of a bluish white colour, resembling small fragments of shells; they were harder than the others, but might easily be crumbled between the fingers into a gritty powder, and when applied to the tongue and palate had a slight saline testaceous taste: they did not exceed in weight 4 grs. The rest were of a cineritious colour, rough on the surface, and more friable; and intermixed with these were some larger, light, spongy particles, somewhat resembling pumice-stones. It is probable that the Arabs, from these appearances of the drug, were led into the opinion already mentioned of its production. The 2 middle joints were of a pure white colour within, and lined with a thin film; it was in these chiefly the Tabasheer was found. The others, particularly the 2 upper joints, were discoloured within, and in some parts of the cavity was found a blackish substance, in grains or in powder, adhering to the sides, the film being there obliterated. In 2 or 3 of the joints, a small round hole was found at top and bottom, which seemed to have been perforated by some insect.

In July, 43 green bamboos, each consisting of 5 or 6 joints, were brought from the hills 50 miles distant from hence. Six, appearing to contain more Tabasheer than the others, were set apart; the remaining 37 were split and

examined in the manner before-mentioned. The result was as follows. In 9 out of the 37 there were no vestiges of Tabasheer. In 28 some were found in 1, 2, or 3 joints of each; but never in more than 3 joints of the same bamboo. The quantity varied, but in all was inconsiderable; and the empty joints were sometimes contiguous, sometimes interrupted, indifferently.

The drug consists of very dissimilar particles at first when taken from the bamboo. The whiter, smooth, harder particles, when not loose together with the others in the cavity, were mostly found adhering to the septum that divides the joints, and to the sides contiguous; but never to the sides about the middle of the joints; and it may be remarked that, instead of being chiefly found at the lower extremity of the joint, as might be expected from the juice settling there, they were found adherent indifferently to either extremity, and sometimes to both. In this situation they formed a smooth lining, somewhat resembling polished stucco, which usually was cracked in several places, and might easily be detached with a blunt knife. In some joints the Tabasheer was found thus collected at one or both extremities only, and in such no rattling was perceived on shaking the bamboo; but generally, while some adhered to the extremities of the joint, other detached pieces were intermixed with the coarser loose particles in the cavity. The quantity found in each bamboo was very inconsiderable; the produce of the whole 28 reeds, from 5 to 7 feet long, not much exceeding 2 drams. It is remarked by Garzius, that the Tabasheer is not found in all bamboos, nor in all the branches indiscriminately; but only in those growing about Bisnagur, Batecala, and one part of the Malabar Coast. From the inconsiderable quantity procured from 28 bamboos, it seems very probable that, though not absolutely confined to certain regions, it may be produced in greater abundance in some soils than in others; but that in all regions where the bamboo grows favourably, some proportion of the drug will be found, however it may vary in quality or quantity.

Rumphius on this subject refers to Garzius, candidly acknowledging that he had not himself had opportunities of making particular inquiry. Dr. R. expected answers from Ceylon to some queries sent thither some time ago; and in respect to Bisnagur, had been lately informed in a letter from Hydrabad, from a medical gentleman attending the present embassy to the Nizam, "That though Tabasheer be in great request at Hydrabad, and bears a high price, it is never brought thither from Bisnagur; that some of what is found in the Bazars is brought from the Atcour pass in Canoul, and some from Emnabad at the distance of about 80 miles to the N. W.; but that the greatest part comes from Masulipatam. That there are 2 sorts sold in the Bazars; one at the rate of a rupee a dram; the other, of inferior quality, at half the price; but that this is said to be chiefly composed of burnt teeth and bones. That he was informed by a Persee, who had been in Bengal, that the Tabasheer was produced in great

quantities at Sylhet, where it is sold by the pound from 1 rupee to 1¼, and formed a considerable article of trade from Bengal to Persia and Arabia."

N° 3 is a specimen of the prime sort from Hydrabad. It differs materially from the others, not only in its superior whiteness, and the being less mixed with impure particles; but in the being much harder than the purest particles of Dr. R.'s specimens, much heavier, and hardly in any degree friable to the finger. Submitting the specimens to examination, he refrains from experiments on them, which may more successfully be made in England, and proceeds to offer a few observations on the juice of the recent bamboo supposed to form the Tabasheer. Rumphius remarks in Amboina, "Juniores arundines plerumque in inferioribus suis nodis semi-repletæ utcunque sunt limpida aqua potabili, quæ hisce in terris sensim evanescit, in aliis vero regionibus exsiccatur in substantiam albam et calceam, quæ Tabaxir vocatur." Garzius gives an account somewhat different from this. "Fra tutti gli intermezzi de' nodi, si genera un certo liquore dolce e grosso, e ridotto in guisa di farina d' amido, e della istessa bianchezza, et alle volte se ne genera assai, alle volte poco, ma non tutte le canne, nè meno tutti i rami generano tale humore..... Questo liquore dopo d' essere appreso, mostra d' essere di color nero, over cinericcio, e non perciò é tenuto per tristo, imperoche questo avvienne, ò perche sia troppo humido, ò perche sia stato lungo tempo nel legno rinchiuso, si come s' hanno pensato alcuni: conciosia che in molti rami, che non sono stati toccati dal fuoco, intravenga, questo."*

The existence of this fluid in the bamboo is known by shaking the joint. In a considerable number of bamboos split in order to procure it, Dr. R. never found water in more than 2 joints, and generally not more than 2 or 3 drs. in each; the largest quantity procured at one time was 1¼ oz. Very few joints in proportion contained any. The fluid was always transparent, but varied in consistence; when thicker it had a whiter colour than common; when more dilute it differed little to the eye from common water, or sometimes had a pale greenish cast. Applied to the tongue and palate, it had a slight saline, sub-astringent taste, more or less perceptible in proportion to the consistence of the fluid. After evaporation in the sun, the residuum had a pretty strong saline taste, with less astringency. Some of the fluid, of a darkish colour, thickened in the reed to the consistence of honey; and some, in another joint of the same reed, was perfectly white and almost dry: both had the sharp salt taste, which the Tabasheer itself loses in a great degree by keeping.

From 2 green bamboos, each of 5 joints, which had been cut only a few days before, he procured above 2 oz. of fluid; it had a slight saline taste, and in colour had a greenish cast. One oz. was put into a phial, N° 1, and about

* Capitolo XII.—Orig.

10 drs. into another phial, N° 2; both were stopped with glass stoppers. After 2 days they had both deposited a small sediment; but the sediment in N° 1 was 3 times more than that in the other. At the end of the week, the water in both was found sweet, and the sediment increased, but most in N° 1. At the end of a fortnight, the water in N° 1 had a fetid smell, with a whitish cottony sediment, and a thin film of the same kind suspended at top. The whole, well shaken together, was poured into a glass vessel, and left to evaporate slowly. The residuum consisted of small particles of a whitish brown colour, resembling the inferior sort of Tabasheer. The water in N° 2 had hardly any fetid smell at this time; and at the end of the month remained in the same state: the sediment had increased very little.

The recent green bamboos which, on shaking, appeared to contain water in the cavity, lost this appearance after standing a few days, some sooner, some later. When split, after they no longer gave any sound by shaking, sometimes no fluid was found in the cavity, as if the whole had escaped. The interior thin pellicle however was discoloured, as if by recent moisture; but generally some of the fluid remained in a mucilaginous state, more or less thick, at the lower part of the joint. It may be remarked that small worms were sometimes found in the same joints with the water, which survived several hours, swimming about in the water after its extraction.

In the latter end of Oct. a green bamboo of 5 joints was brought to him, which appeared to contain both water and Tabasheer. After 3 days, the sound of water on shaking the reed, could hardly be perceived; on the 5th day it was entirely imperceptible. On splitting the bamboo, about $\frac{1}{4}$ dr. of the fluid, now thickened into a mucilage, was found at the bottom of the upper joint. The 2d joint contained some perfect Tabasheer loose in the cavity. The 3d joint was empty, excepting a few particles of Tabasheer, which adhered to the sides near the bottom. The 4th joint, at the bottom, contained above 1 dr. of a brownish pulpy substance, adherent. The last joint, in like manner, contained $\frac{1}{4}$ dr. of a substance thicker and harder in consistence, and nearly of the colour of white wax. This specimen exhibited at one view the progress of the Tabasheer through its several stages. The sound distinctly perceived in the first joint on the 23d of Oct. was produced by the water in a fluid state; on the 31st, having become thicker, the sound, on shaking, was very obscure; on the 2d of Nov. no sound was perceptible; and when the reed was split, the water was found reduced to a mucilage. The 4th and 5th joints contained the drug in a more advanced state. In the first, it was thicker than a mucilage of a brownish colour; in the 2d, more of the fluid part having evaporated, the colour was whiter, and it wanted but little of the consistence of the perfect Tabasheer found in the 2d joint.

P. S. Four of the 7 reeds presented to the Society on the night this paper was

read, being carefully split, the contents, on comparing them with the specimen sent from India, then on the table, were found to agree in all respects, as well as with the description of the more recent drug given in the above paper. The specimen, N° 3, sent from Hydrabad, and reckoned the prime sort, differed somewhat in hardness, as mentioned above, from the purest particles in the Tabasheer collected by himself; but in the opinion of several of the members present, who compared them, were the same substance with the particles mixed, in a small proportion, in some of the other specimens, as likewise with a few particles taken from the reeds opened in their presence; which puts it beyond doubt, that the substance is produced in the cavity of the bamboo.

XIV. Of the Nardus Indica, or Spikenard. By Gilbert Blane, M.D., F.R.S. p. 284.

Dr. B. received an account, some time ago from his brother in India, of the Spikenard, or Nardus Indica, a name familiar in the works of the ancient physicians, naturalists, and poets: but the identity of which has not hitherto been satisfactorily ascertained. His brother writes, in a letter dated Lucknow, Dec. 1786, that, " travelling with the Nabob Visier, on one of his hunting excursions towards the northern mountains, I was surprized one day, after crossing the river Rapty, about 20 miles from the foot of the hills, to perceive the air perfumed with an aromatic smell; and, on asking the cause, I was told it proceeded from the roots of the grass that were bruised or trodden out of the ground by the feet of the elephants and horses of the Nabob's retinue. The country was wild and uncultivated, and this was the common grass which covered its surface, growing in large tufts close to each other, very rank, and in general from 3 to 4 feet in length. As it was the winter season, there was none of it in flower. Indeed the greatest part of it had been burnt down on the road we went, in order that it might be no impediment to the Nabob's encampments. I collected a quantity of the roots to be dried for use, and carefully dug up some of it, which I sent to be planted in my garden at Lucknow. It there throve exceedingly, and in the rainy season it shot up spikes about 6 feet high. This is accompanied with a drawing of the plant in flower, and of the dried roots, in which the natural appearance is tolerably preserved. It is called by the natives Terankus, which means literally, in the Hindoo language, fever-restrainer, from the virtues they attribute to it in that disease. They infuse about a dram of it in half a pint of hot water, with a small quantity of black pepper. This infusion serves for one dose, and is repeated 3 times a day. It is esteemed a powerful medicine in all kinds of fevers, whether continued or intermittent. I have not made any trial of it myself; but shall certainly take the first opportunity of doing so. The whole plant has a strong aromatic odour; but both the smell and the virtues reside

principally in the husky roots, which in chewing have a bitter, warm, pungent taste, accompanied with some degree of that kind of glow in the mouth which cardamoms occasion."

Besides the drawing, a dried specimen has been sent, which was in such good preservation as to enable Sir Joseph Banks, P. R. S., to ascertain it by the botanical characters to be a species of Andropogon, different from any plant that has usually been imported under the name of Nardus, and different from any of that genus hitherto described in botanical systems. There is great reason however to think, that it is the true Nardus Indica of the ancients; for first, the circumstance, in the account above recited, of its being discovered in an unfrequented country from the odour it exhaled by being trodden on by the elephants and horses, corresponds, in a striking manner, with an occurrence related by Arrian, in his History of the Expedition of Alexander the Great into India. It is there mentioned, lib. 6, cap. 22, that during his march through the desarts of Gadrosia, the air was perfumed by the Spikenard, which was trampled under foot by the army; and that the Phœnicians, who accompanied the expedition, collected large quantities of it, as well as of myrrh, in order to carry them to their own country, as articles of merchandize. This last circumstance seems further to ascertain it to have been the true Nardus; for the Phœnicians, who even in war appear to have retained their genius for commerce, could no doubt distinguish the proper quality of this commodity. Dr. B. was informed by Major Rennell, F. R. S., whose accurate researches in Indian geography are so well known to the public, that Gadrosia or Gedrosia answers to the modern Mackran or Kedge-Mackran, a maritime province of Persia, situated between Kerman (the ancient Carmania) and the river Indus, being of course the frontier of Persia towards India; and that it appears from Arrian's account, and from a Turkish map of Persia, that this desart lies in the middle of the tract of country between the river Indus and the Persian Gulph, and within a few days' march of the Arabian or Erythræan sea*. It would appear, that the Nard was found towards the eastern part of it; for Alexander was then directing his route to the westward, and the length of march through the desart afterwards was very great, as they were obliged to kill their beasts of burden in consequence of their subsequent distress. 2dly, Though the accounts of the ancients concerning this plant are obscure and defective, it is evident that it was a plant of the order of gramina; for the term arista, so often applied to it, was appropriated by them to the fructification of grains and grasses, and seems to be a word of Greek original to denote the most excellent portion of these plants, which are the most

* By the Erythræan Sea the ancients meant the northern part of the Æthiopic Ocean, washing the southern coasts of Arabia and Persia, and not, as the name would imply, what is, in modern times, called the Red Sea. The ancient name of the Red Sea was Sinus Arabicus.—Orig.

useful in the vegetable creation for the sustenance of animal life, and Nature has also kindly made them the most abundant in all parts of the habitable earth. The term spica is applied to plants of the natural order verticillatæ, in which there are many species of fragrant plants, and the lavender, which being an indigenous one, affording a grateful perfume, was called Nardus Italica by the Romans; but we never find the term arista applied to these. The poets, as well as the naturalists, constantly apply this latter term to the true Nardus. Statius calls the Spikenard odoratæ aristæ. Ovid, in mentioning it as one of the materials of the Phœnix's nest, calls it Nardi levis arista; and a poem, ascribed to Lactantius, on the same subject, says, his addit teneras Nardi pubentis aristas, where the epithet pubentis seems even to point out that it belonged to the genus andropogon, a name given to it by Linnæus from this circumstance. Galen says, that though there are various sorts of Nardus, the term Ναρδοσταχυς, or Spikenard, should not be applied to any but the Nardus Indica. It would appear that the Nardus Celtica was a plant of a quite different habit, and is supposed to be a species of Valeriana. The description of the Nardus Indica by Pliny does not indeed correspond with the appearance of our specimen; for he says it is frutex radice pingui et crassâ; whereas ours has small fibrous roots. But as Italy is very remote from the native country of this plant, it is reasonable to suppose that others, more easily procurable, used to be subtsituted for it; and the same author says, that there were 9 different plants by which it could be imitated and adulterated. There would be strong temptations to do this from the great demand for it, and the expense and difficulty of distant inland carriage; and as it was much used as a perfume, being brought into Greece and Italy in the form of an unguent manufactured in Laodicea, Tarsus, and other towns of Syria and Asia Minor, it is probable that any grateful aromatic resembling it was allowed to pass for it. It is probable that the Nardus of Pliny, and great part of what is now imported from the Levant, and found under that name in the shops, is a plant growing in the countries on the Euphrates, or in Syria, where the great emporiums of the eastern and western commerce were situated. There is a Nardus Assyria mentioned by Horace; and Dioscorides mentions the Nardus Syriaca, as a species different from the Indica, which certainly was brought from some of the remote parts of India; for both Dioscorides and Galen, by way of fixing more precisely the country whence it comes, call it also Nardus Gangites. 3dly, Garcias ab Horto, a Portuguese, who resided many years at Goa in the 16th century, has given a figure of the roots, or rather the lower parts of the stalks, which corresponds with our specimen; and he says expressly, that there is but this one species of Nardus known in India, either for the consumption of the natives, or for exportation to Persia and Arabia. It is remarkable that he is perhaps the only author who speaks of it in its recent state from his own observation.

It is not to be met with among the many hundreds of plants delineated in the Hortus Malabaricus. The Schœnanthus of Rumphius does not correspond with it, being only one palm in height; but he mentions having seen a dried specimen of it, of which the leaves were almost 5 feet high; and that Mackran was one of the countries whence it was brought. This must be the same as that mentioned by Arrian, but differs from that of Garcias in the length of the stalks; but this might be either because the measure was taken at different seasons of the year, for the specimen before us was much shorter in winter than when it shot into spikes, or because that of Garcias being, according to his own account, cultivated, it might not be so luxuriant as that which grew spontaneous in its native soil. 4thly, The sensible qualities of this are superior to what commonly passes for it in the shops, being possessed both of more fragrancy and pungency, which seems to account for the preference given to it by the ancients.

There is a question concerning which Mathiolus, the commentator of Dioscorides, bestows a good deal of argument, viz. whether the roots or stalks were the parts esteemed for use, the testimony of the ancients themselves on this point being ambiguous. The roots of this specimen are very small, and possess sensible qualities inferior to the rest of the plant; yet it is mentioned in the account above recited, that the virtues reside principally in the husky roots. It is evident, that by the husky roots must here be meant the lower parts of the stalks and leaves where they unite to the roots; and it is probably a slight inaccuracy of this kind that has given occasion to the ambiguity that occurs in the ancient accounts.

With regard to the virtues of this plant, it was highly valued anciently as an article of luxury as well as a medicine. The favourite perfume which was used at the ancient baths and feasts was the unguentum nardinum; and it appears, from a passage in Horace, that it was so valuable, that as much of it as could be contained in a small box of precious stone was considered as a sort of equivalent for a large vessel of wine, and a handsome quota for a guest to contribute at an entertainment, according to the custom of antiquity:

——Nardo vina merebere
Nardi parvus onyx eliciet cadum.

It may here be remarked, that as its sensible qualities do not depend on a principle so volatile as essential oil, like most other aromatic vegetables, this would be a great recommendation to the ancients, as its virtues would thus be more durable, and they were not acquainted with the method of collecting essential oils, being ignorant of the art of distillation. The fragrance and aromatic warmth of the Nardus depends on a fixed principle like that of cardamoms, ginger, and some other species. Dr. B. tried to extract the virtues of the Nardus by boiling water, by maceration in wine and in proof spirits, but it yielded them but sparingly and with difficulty to all these menstrua.

It had a high character among the ancients as a remedy both external and internal. It is one in the list of ingredients in all the antidotes, from those of Hippocrates, as given on the authority of Myrepsus and Nicolaus Alexandrinus, to the officinals which have kept their ground till modern times under the names of Mithridate and Venice Treacle. It is recommended by Galen and Alexander Trallian in the dropsy and gravel. Celsus and Galen recommended it both externally and internally in pains of the stomach and bowels. The first occasion on which the latter was called to attend Marcus Aurelius was when that Emperor was severely afflicted with an acute complaint in the bowels, answering by the description to what we now call cholera morbus; and the first remedy he applied was warm Oleum nardinum on wool to the stomach. He was so successful in the treatment of this illness, that he ever afterwards enjoyed the highest favour and confidence of the Emperor. It would appear, that the natives of India consider it as an efficacious remedy in fevers, and its sensible qualities promise virtues similar to those of other simples now in use among us in such cases. Besides a strong aromatic flavour, it possesses a pungency to the taste little inferior to the serpentaria, and much more considerable than the contrayerva. It is mentioned in a work attributed to Galen, that a medicine, composed of this and some other aromatics, was found useful in long protracted fevers, which are the cases in which medicines of this class are employed in modern practice. Pl. 8, fig. 1, is a representation of the plant.

XV. On some Extraordinary Effects of Lightning. By Wm. Withering, M.D., F.R.S. p. 293.

This thunder cloud formed in the south, in the afternoon of Sept. 3, 1789, and took its course nearly due north. In its passage it set fire to a field of standing corn; but the rain presently extinguished the fire. Soon afterwards the lightning struck an oak tree, in the Earl of Aylesford's park at Packington. The height of this tree is 39 feet, including its trunk, which is 13 feet. It did not strike the highest bough, but that which projected farthest southward. A man, who had taken shelter against the north side of the tree, was struck dead instantaneously, his clothes set on fire, and the moss (lichen) on the trunk of the tree, where the back of his head had rested, was likewise burnt. Two men, spectators of the accident, ran immediately towards him on seeing him fall; and as it rained hard, and a small lake had collected almost close to the spot, the fire was very soon extinguished; but the effects of the fire on one-half of his body, and on his clothes, were such as to show that the whole burning was instantaneous, not progressive.

Part of the electric matter passed down a walking stick, which the man held in his hand, sloping from him; and where the stick rested on the ground, it made a perforation about $2\frac{1}{2}$ inches in diameter, and 5 inches deep. All obser-

vation would probably have ended here, had not Lord Aylesford determined to erect a monument on the spot, not merely to commemorate the event, but with an inscription, to caution the unwary against the danger of sheltering under a tree during a thunder storm. In digging the foundation for this monument, the earth was disturbed at the perforation before mentioned, and the soil appeared to be blackened to the depth of about 10 inches. At this depth, a root of the tree presented itself, which was quite black; but this blackness was only superficial, and did not extend far along it. About 2 inches deeper, some melted quartose matter began to appear, and continued in a sloping direction to the depth of 18 inches. Mr. Watt suggested the idea, that the hollows had been occasioned by the expansion of moisture while the fusion existed.

XVI. Account of a Child with a Double Head. By Everard Home, Esq., F. R. S. p. 296.

It is much to be regretted, that the histories of monstrous appearances in the structure of the human body which are to be found in the works of the older writers, and even of many of the moderns, are so little to be depended on. Few authors have contented themselves with giving a simple detail of facts that were extraordinary; but, from an over anxiety to make them still more wonderful, or from having given an implicit belief to the accounts received from the credulous and ignorant, they have commonly added circumstances too extravagant to deserve the attention of a reasonable mind, which prevent the reader from giving credit to any part of the narration. This has been so general, that whenever the history of any thing uncommon appears, the mind is impressed with a doubt of its authenticity, and requires some stronger evidence of the facts than the single testimony of an individual in other respects unimpeached in his veracity.

As the histories of remarkable deviations from the common course of nature in the formation of the human body, already registered in the Philos. Trans. are very numerous, Mr. H. is desirous of adding to them an account of one so truly uncommon, that no similar instance is to be found on record. It is a species of lusus naturæ so unaccountable, that, though the facts are sufficiently established by the testimonies of the most respectable witnesses, he should still be diffident in bringing them before the R. S., were he not enabled at the same time to produce the double skull itself, in which the appearances illustrate so clearly the different parts of the history that it must be rendered perfectly satisfactory to the minds of the most incredulous.

The child was born in May, 1783, of poor parents; the mother was 30 years old, and named Nooki; the father was called Hannai, a farmer at Mandalgent near Bardawan, in Bengal, and aged 35. At the time of the child's birth, the

woman who acted as midwife, terrified at the strange appearance of the double head, endeavoured to destroy the infant by throwing it on the fire, where it lay a sufficient time, before it was removed, to have one of the eyes and ears considerably burnt. The body of the child was naturally formed, but the head appeared double, there being, besides the proper head of the child, another of the same size, and to appearance almost equally perfect, attached to its upper part. This upper head was inverted, so that they seemed to be 2 separate heads united together by a firm adhesion between their crowns, but without any indentation at their union, there being a smooth continued surface from the one to the other. The face of the upper head was not over that of the lower, but had an oblique position, the centre of it being immediately above the right eye. When the child was 6 months old, both of the heads were covered with black hair, in nearly the same quantity. At this period the skulls seemed to have been completely ossified, except a small space between the ossa frontis of the upper one, like a fontinel.

No pulsation could be felt in the situation of the temporal arteries; but the superficial veins were very evident. The neck was about 2 inches long, and the upper part of it terminated in a rounded soft tumor, like a small peach. One of the eyes had been considerably hurt by the fire, but the other appeared perfect, having its full quantity of motion; but the eyelids were not thrown into action by any thing suddenly approaching the eye; nor was the iris at those times in the least affected; but, when suddenly exposed to a strong light, it contracted though not so much as it usually does. The eyes did not correspond in their motions with those of the lower head; but appeared often to be open when the child was asleep; and shut when it was awake. The external ears were very imperfect, being only loose folds of skin; and one of them mutilated by having been burnt. There did not appear to be any passage leading into the bone which contains the organ of hearing. The lower jaw was rather smaller than it naturally should be, but was capable of motion. The tongue was small, flat, and adhered firmly to the lower jaw, except for about half an inch at the tip, which was loose. The gums in both jaws had the natural appearance; but no teeth were to be seen either in this head or the other. The internal surfaces of the nose and mouth were lubricated by the natural secretions, a considerable quantity of mucus and saliva being occasionally discharged from them. The muscles of the face were evidently possessed of powers of action, and the whole head had a good deal of sensibility, since violence to the skin produced the distortion expressive of crying, and thrusting the finger into the mouth made it show strong marks of pain. When the mother's nipple was applied to the mouth, the lips attempted to suck. The natural head had nothing uncommon in its appearance; the eyes were attentive to objects, and its mouth sucked the breast vigorously. Its body was emaciated.

The parents of the child were poor, and carried it about the streets of Calcutta as a curiosity to be seen for money; and to prevent its being exposed to the populace, they kept it constantly covered up, which was considered as the cause of its being emaciated and unhealthy. The attention of the curious was naturally attracted by so uncommon a species of deformity; and Mr. Stark, who resided in Bengal during this period, paid particular attention to the appearances of the different parts of the double head, and endeavoured to ascertain the mode in which the 2 skulls were united, as well as to discover the sympathies which existed between the 2 brains. On his return to England, finding that Mr. H. was in possession of the skull, and proposed drawing up an account of the child, he favoured him with the following particulars, and likewise allowed him to have a sketch taken from a very exact painting, made under his own inspection from the child while alive, by Mr. Smith, a portrait painter then in India. From this drawing, which is annexed, and 2 others, representing the heads in the natural state; and the skulls, when all the other parts were removed, a much more accurate idea will be given of the child's appearance than can be conveyed by any description.

At the time Mr. Stark saw the child, it must have been nearly 2 years old,* as it was some months before its death, which it appears happened in the year 1785. At this period the appearances differed in many respects from those taken notice of when only 6 months old. The burnt ear had so much recovered itself as only to have lost about $\frac{1}{4}$ part of the loose pendulous flap. The openings leading from the external ear appeared as distinct as in those of the other head. The skin surrounding the injured eye, which was on the same side with the mutilated ear, was in a slight degree affected, and the external canthus much contracted, but the eye itself was perfect. The eyelids of the superior head were never completely shut, remaining a little open, even when the child was asleep, and the eyeballs moved at random. When the child was roused, the eyes of both heads moved at the same time; but those of the superior head did not appear to be directed to the same object, but wandered in different directions. The tears flowed from the eyes of the superior head almost constantly, but never from the eyes of the other, except when crying. The termination of the upper neck was very irregular, a good deal resembling the cicatrix of an old sore. The superior head seemed to sympathise with the child in most of its natural actions. When the child cried, the features of this head were affected in a similar manner, and the tears flowed plentifully. When it sucked the mother, satisfaction was expressed by the mouth of the superior head, and the saliva flowed more copiously than at any other time; for it always flowed a little from it. When the child

* The dentes molares, or double teeth, which usually appear at 20 months or 2 years of age, were through the gum; and there was no reason to expect them very early in this child.—Orig.

smiled, the features of the superior head sympathised in that action. When the skin of the superior head was pinched, the child seemed to feel little or no pain, at least not in the same proportion as was felt from a similar violence being committed on its own head or body.

When the child was about 2 years old, and in perfect health, the mother went out to fetch some water; and on her return found it dead, from the bite of a cobra de capelo. The parents at this time lived on the grounds of Mr. Dent, the honourable East India Company's agent for salt at Tumloch, and the body was buried near the banks of the Boopnorain river. It was afterwards dug up by Mr. Dent and his European servant, the religious prejudices of the parents not allowing them to dispense with its being interred. The double skull was brought to Europe by Capt. Buchanan, late commander of the Ranger packet, in the service of the honourable the East India Company, and deposited by Mr. Home in Mr. John Hunter's curious collection.

The 2 skulls which compose this monstrous head appear to be nearly of the same size, and equally complete in their ossification, except a small space at the upper edge of the ossa frontis of the superior skull, similar to a fontinel. The mode in which the 2 are united is curious, as no portion of bone is either added or diminished for that purpose; but the frontal and parietal bones of each skull, instead of being bent inwards, so as to form the top of the head, are continued on; and, from the oblique position of the 2 heads, the bones of the one pass a little way into the natural sutures of the other, forming a zig-zag line, or circular suture uniting them together. The 2 skulls appear to be almost equally perfect at their union; but the superior skull, as it recedes from the other, is becoming more imperfect and deficient in many of its parts. The meatus auditorius in the temporal bone is altogether wanting. The basis of the skull is imperfect in several respects, particularly in such parts as are to connect the skull with a body. The foramen magnum occipitale is a small irregular hole, very insufficient to give passage to a medulla spinalis; round its margin are no condyles with articulating surfaces, as there were no vertebræ of the neck to be attached to it. The foramen lacerum in basi cranii is only to be seen on one side, and even there too small for the jugular vein to have passed through. The ossa palati are deficient at their posterior part; the lower jaw is too small for the upper, and the condyle and coronoid process of one side are wholly wanting. In most of the other respects, the 2 skulls are alike; the number of teeth in both is the same, viz. 16.

From an examination of the internal structure of the double skull, the 2 brains have certainly been inclosed in 1 bony case, there being no septum of bone between them. How far they were entirely distinct, and surrounded by their proper membranes, cannot now be ascertained; but from the sympathies which were

noticed by Mr. Stark between the 2 heads, more particularly those of the superior with the lower, or more perfect, Mr. H. believes that there was a more intimate connection between them than simply by means of nerves, and therefore that the substance of the brains was continued into each other. Had the child lived to a more advanced age, and given men of observation opportunities of attending to the effects of this double brain, its influence on the intellectual principle must have afforded a curious and useful source of inquiry; but unfortunately the child only lived long enough to complete the ossification of the skull so as to retain its shape, by which means we have been enabled to ascertain and register the fact, without having enjoyed the satisfaction that would have resulted from an examination of the brain itself, and a more mature investigation of the effects it would have produced.

In pl. 8, fig. 2, the child is represented as it appeared at the age of 20 months, and is copied from a picture in the possession of Mr. Stark. The painting was taken from the child 6 months before its death by Mr. Smith, an ingenious artist, at that time residing in Bengal. It conveys a general idea of the appearance of this extraordinary child, and the relative proportions between the double head and the body. In fig. 3, the double head is represented of a larger size. One of the eyes of the upper face appears smaller or more contracted than the other; this is in consequence of the injury it received when the child was thrown upon the fire. The superficial veins on the forehead of the upper head are very distinctly seen. Fig. 4 is an exact representation of the double skull, which is in Mr. Hunter's collection, upon the same scale. It shows the curious manner in which the 2 skulls are united together, and the number of teeth formed before the child's death; which circumstance ascertains, with tolerable accuracy, its age.

XVII. On the Analysis of a Mineral Substance from New South Wales. By Josiah Wedgwood, Esq., F.R.S., and A.S.* p. 306.

This mineral is a mixture of fine white sand, a soft white earth, some colourless micaceous particles, and a few black ones resembling black mica or black-lead; partly loose or detached from each other, and partly cohering together in little friable lumps. None of these substances seem to be at all acted on by the nitrous acid, concentrated or diluted; nor by oil of vitriol diluted with about equal its measure of water; in the cold, or in a boiling heat; the mineral remained unaltered in its appearance, and the acids had extracted nothing from it that could be precipitated by alkali.

Oil of vitriol boiled on the mineral to dryness, as in the process of making alum from clay, produced no apparent change in it; but a lixivium made from this dry mass with water, on being saturated with alkali, became somewhat turbid, and deposited, exceeding slowly, a white earth in a gelatinous state, too

* From Sydney Cove, New South Wales. Along with the mineral here analyzed, Mr. W. was presented by Sir Joseph Banks with some clay from Sydney Cove, which Mr. W. found to be an excellent material for pottery, adding that it might certainly be made the basis of a valuable manufacture for our infant colony there.

small in quantity for any particular examination; but which, from its aspect, from the manner in which it was obtained, and from the taste of the lixivium before the addition of the alkali, was judged to be the aluminous earth. The marine acid, during digestion, seemed to have as little action as the other 2; but on pouring in some water, with a view only to dilute and wash out the remaining part of the acid, a remarkable difference presented itself; the liquor became instantly white as milk, with a fine white curdly substance intermixed; the strong acid having extracted something which the simple dilution with water precipitated.

The white matter being washed off, more spirit of salt was added to the remainder, and the digestion repeated, with a long tube inserted into the mouth of the glass, so as nearly to prevent evaporation. The acid, when cold and settled fine, was poured off clear; and on diluting it with water, the same milky appearance was produced as at first. The digestion was repeated several times successively, with fresh quantities of the acid, till no milkiness appeared on dilution. The quantity of mineral employed was 24 grs.; and the residuum, after the operations, washed and dried, weighed somewhat more than 19 grains; so that about $\frac{1}{5}$ of it had been dissolved. In some parcels of the mineral, taken up promiscuously, the proportion of soluble matter was much less, and in none greater. It is only the white part, and only a portion of this, that the acid appears to act on: the white sand, much of the white soft earth, and all the black particles, remain unaltered.

To try whether this tedious process of solution could be expedited by trituration or calcination, some of the mineral was rubbed in a mortar; and in doing this, it appeared pretty remarkable, that though the black part bore but an inconsiderable proportion to the rest, yet the whiteness of the other was soon covered and suppressed by it, the whole becoming a uniformly black, shining, soft, unctuous mass, like black-lead rubbed in the same manner; with a few gritty particles perceptible on pressing hard with the pestle. A penny-weight of this mixture, spread thin on the bottom of a porcelain vessel, was calcined about an hour, with a fire between 30 and 40 degrees;* it became of a uniform, dull, white, or grey colour, excepting a very few, and very small, sparkling, black particles, suspected to be those which had eluded the action of the pestle; it lost in weight 6 grs. or a 4th part. The mineral, thus ground and calcined, was found to be just as difficult of solution as in its crude state; with this additional disadvantage, that the undissolved fine particles are indisposed to settle from the liquor.

* By degrees of fire, or of heat above ignition, I mean those of my thermometer; and some idea may be formed of their value, by recollecting, that they commence at visible redness; and that the extreme heat of a good air-furnace, of the common construction, is 160°, or a little more.—Orig.

1

In all the experiments of dissolution, as often as the heat was at or near the boiling point of the acid, frequent and pretty singular bursts or explosions happened, though the matter lay very thin in a broad-bottomed glass. They were sometimes so considerable as to throw off a porcelain cup with which the glass was covered, and once to shatter the glass in pieces. In a heat a little below this, the extraction seemed to be equally complete, though more slow; but a heat a little below that in which wax melts, or below 140° of Fahrenheit's thermometer, appeared insufficient.

To determine the degree of dilution necessary for the precipitation of the dissolved substance, and whether the precipitation by water be total, a measure of the solution was poured into a large glass, and the same measure of water added repeatedly. The 3d addition of water occasioned a slight milkiness, which increased more and more to the 6th. The liquor being then filtered off, another measure of water produced a little fresh milkiness, and an 8th rather increased it; a 9th and a 10th had no effect. The liquor being now again passed through a filter, solution of salt of tartar did not in the least alter its transparency; so that, after the solution has been diluted with 8 or 9 times its measure of water, there is nothing left in it that alkali can precipitate.

From the manner in which the solution is necessarily prepared, it cannot but contain a great redundance of acid; for the small quantity of acid, sufficient for holding the soluble part suspended, would be soaked up or entangled by the undissolved part, so as scarcely to admit of any being poured off; and it cannot be diluted, or washed out, but by the strong acid itself. The solution with which the above experiment was made was reckoned to have only about 6 grs. of the soluble matter to 3 oz. of spirit of salt, having been prepared by digesting that quantity of the spirit by half an ounce at a time on 30 grs. of the crude mineral. A saturated solution was obtained by digesting, in a small portion of the solutions thus prepared, the precipitate thrown down by water from the larger portions, till the acid would take up no more. A solution thus saturated cannot bear the smallest quantity of water, a single drop, on the first contact, producing a milky circle round it.

This substance, washed and dried, is indissoluble in water, as indeed might be expected from the manner of its preparation. Nor is it acted on by the nitrous or vitriolic acids, concentrated or diluted, cold or hot; nor by alkaline solutions, mild or caustic, of the volatile or fixed kind. It is dissolved by strong marine acid, but not without the assistance of nearly the same degree of heat that is necessary for its extraction from the mineral. From this solution it is precipitated by water; and, after repeated dissolutions and precipitations, it appears to have suffered no decomposition or change.

Spirit of nitre, added to the saturated solution, makes no precipitation; and if

the quantity of nitrous acid exceeds, or at least does not fall much short of, that of marine acid in the solution, the mixture suffers no precipitation from water. Nor does any precipitation happen, though the nitrous spirit be previously mixed with even a large quantity of water; provided the quantity of solution added to it does not exceed that of the nitrous spirit in the mixture. The appropriate menstruum for this substance, that is for keeping it in a state of dilute solution, appears therefore to be aqua regia; and the due proportions of the two acids, of any given strength, might be determined, if necessary, with greater accuracy and facility for this than perhaps for any other body; because, if there be even a very minute surplus of marine acid in the solution, that surplus will instantly betray itself on dropping a little into water, all that was dissolved by it, and no more, being precipitated by the water. It may be observed however, that where an addition of nitrous acid is used, a saturated solution cannot be obtained, unless by subsequent evaporation, the same quantity of marine acid being necessary with as without that addition: the change, or modification, which the nitrous acid produces in the marine, serves, in the present instance, not for effecting the solution, as in the case of gold and some other metals, but merely for enabling it to bear water without depositing its contents.

Oil of vitriol, dropped into the saturated marine solution, occasions no change till its quantity comes to be about equal to that of the solution; a considerable effervescence and heat are then produced, the liquor becomes milky, and the marine acid is extricated in its usual white fumes. The mixture, heated nearly to boiling, becomes transparent, and afterwards continues so in the cold. This vitriolic solution is precipitated by water, and the precipitate is re-dissolved by marine acid. The saturated marine solution is indisposed to crystallize. By continued evaporation in gentle heat, it becomes thick and butyraceous, and in this state it soon liquefies again on exposure to the air. The butyraceous mass, in colour whitish or pale yellow, is not corrosive, like the similar preparations made from some metallic bodies; nor is it more pungent in taste, but rather less so than the combination of the same acid with calcareous earth. In a heat increased nearly to ignition, the acid is disengaged, and rises in white fumes, which, received in a cold phial, condense into colourless drops, without any appearance of sublimate. From the remaining white mass, spirit of nitre extracts so little as to exhibit only a slight milkiness on adding alkali; a proof that nearly all the marine acid had been expelled; for, while that acid remains, the whole is dissoluble by the nitrous.

The substance in question is not precipitated by Prussian lixivium. A drop or two of the lixivium do indeed occasion a little white or bluish-white precipitation in the saturated marine solution; but in the more dilute no turbidness appears, till the quantity of lixivium is such as to produce that effect by its mere water;

and when the precipitate has at length been formed, it re-dissolves in marine acid as easily as that made by water; whereas the precipitates resulting from the union of the Prussian matter are not acted on by acids, till that matter has been extracted from them by an alkali. For further satisfaction in this important point, the experiment was repeated with a solution in aqua regia. Here the Prussian lixivium, in whatever quantity it was added, occasioned no precipitation at all, only the usual bluishness arising from the iron always found in the common acids; and pure alkali, added afterwards, precipitated the original white substance unchanged.

The following experiments of precipitation by alkalis were made with the marine solution, before the effect of an addition of nitrous acid had been discovered; and they were made with so much care and attention, that it was not thought necessary to repeat them afterwards. To obviate, as much as possible, the equivocal results that might arise from water contained in the precipitants, the different alkalis were applied in the dryest state they could be reduced to; viz. pure salt of tartar, kept for some time in a heat just below redness; crystals of marine alkali, melted and dried in the same manner; volatile alkali in crystals, a little surplus acid being, in this instance, previously added to the solution, to counteract the water of crystallization in the alkali; salt of tartar causticated by quick-lime, and hastily evaporated to dryness; the marine alkali causticated in like manner; and the vapour of caustic volatile alkali arising, with a very gentle heat, from a retort into a phial containing the solution. All these alkalis occasioned copious precipitations. All the precipitates, after washing and drying, were found to re-dissolve in marine acid; and from all these solutions the original substance was precipitated, unaltered, on diluting them with water.

In strong fire, from 142 to 156°, this substance discovers a much greater fusibility than any of the known simple earths. In a small vessel, made of tobacco-pipe clay, it melted, and glazed the bottom; and on a bed of powdered flint, pressed smooth in the manner of a cupel, it did the same. Magnesia, or chalk, would indeed vitrify in the clay vessel; but on flint, no one of the known earths shows any tendency to vitrification in that heat.* In a cavity, scooped in a lump of chalk, this substance, in the heat above mentioned, ran

* It may be proper just to mention, that I find this to be a very commodious and sure method of trying, in small, whether any given earthy body be fusible with other earths. If the body is disposed to vitrify with any proportion of clay or flint, for instance, it will equally vitrify when a little of it is applied, or even dusted only, on the bottom of a small cup made of clay, or on a smooth close bed of finely powdered flint. The body, in this mode of application, seems to unite with only just so much of the matter of the substratum as is requisite for their most perfect fusion together, and has nothing else in contact with it, so that no deception can arise; whereas, if mixed with the same matter, there might be no appearance of fusion, unless certain favourable proportions of the two should chance to be hit upon; and even then, if the quantity be small, it would not be certain but that the fusion might have originated from the matter of the crucible.—Orig.

into a small round bead, smooth, whitish, and opaque, not in the least adhering to the calcareous mass. On a bed of powdered quick-lime it formed a brownish scoria, which in great part had sunk into the lime, and seemed to have united with it. On Mr. Henry's magnesia, uncalcined, it melted and sunk in completely, leaving only a slight brownish stain on the surface where it had lain. On beds of the baroselenite and barytic quick-lime, it likewise melted and sunk in, leaving a discoloured spot behind; but whether it really united with the substrata, or only penetrated into their interstices, could not be determined with certainty, on account of the smallness of the quantity of the mineral he had to work on. On a bed of powdered charcoal, in a crucible closely luted, this substance likewise melted; and therefore it may be presumed not to have owed its fusion, in the above experiments, to the same cause to which some of the common simple earths, in certain circumstances, owe theirs, namely, their union with the matter of the vessel or support, that is, with an earth or earths of a different kind from themselves; but to possess a fusibility strictly its own, which takes place in a fire of 150°, or perhaps less.

As charcoal in fine powder assumes a kind of fluidity in the fire, similar to that which powdered gypsum exhibits in a small heat, its surface had changed from concave to horizontal, and the bead had sunk to the bottom; it was rough and black on the outside, and whitish within. On repeating the experiment in a cavity scooped in a piece of charcoal, the result was a blackish bead like the former, only smooth on the outside, with something of metallic brightness, not unlike that of black-lead. Both beads were very light, and had a considerable cavity within. All the internal part was whitish, without the least metallic aspect; and the external glossy blackness appeared to be only the stain which charcoal powder communicates, in strong fire, to some earthy bodies that have a tendency to vitrify. By boiling in concentrated marine acid a part of the beads was dissolved, precipitable as at first by water; but an accident prevented the process from being continued sufficiently to determine whether the whole could be dissolved or not.

By this fusibility in the fire; solubility in one only of the common mineral acids, and parting with the acid in a heat below ignition; precipitability by water, and non-precipitability by Prussian lixivium; this substance is strongly discriminated from the known earths and metallic calces. And as it suffers no decomposition from any of the alkalis, in any of the usual modes of application, it cannot be considered as a combination of any of those earths or calces with any of the known acids; for all the combinations of this kind would, in one or other of the above methods of trial, have had the earth or metal disengaged from the acid. Whether this substance belongs to the earthy or metallic class, he cannot absolutely determine; but is inclined to refer it to the earthy; because, though

brought into perfect fusion, in contact with inflammable matter, and in close vessels, it does not assume the appearance which metallic bodies do in that circumstance.

The black particles, which bore but a very small proportion to the other matter, were in form of shining black scales, very thin, and very light. One grain weight of them, carefully picked out, exposed to a fire which was gradually raised to about 90°, and continued in all about 40 hours, in a vessel loosely covered, was almost wholly dissipated, and what little remained was perfectly white. Marine acid had no effect on it. Fifteen grains of the entire mineral lost, in the same fire, 3 grains. After separating from another portion of the mineral, by washing and otherwise, a considerable quantity of the white matter, 15 grs. of the remainder, continuing of course more than its due proportion of the black, lost 5 grains; so that it seems principally to be the substance on which the blackness depends that is destroyed or dissipated by fire. The same quantity, 15 grains, of common black-lead lost in the same fire above 14 grs. the residuum weighing less than 1 grain. Though no conclusion can be drawn from these experiments respecting the comparative loss of black-lead and the pure black matter of this mineral, on account of the heterogeneous parts intermixed with the latter, the colour of the residua seems to afford a sufficient discrimination between them; that of black-lead being dark reddish brown, but the others purely and uniformly white.

As this substance could not now be supposed to be either iron mica, or the common kind of black-lead, suspicion fell on molybdæna. Mr. W. had not, at that time, had an opportunity of procuring a specimen of molybdæna to compare it with; but from the singular and strongly-marked properties of the molybdænic acid, discovered by Scheele, it was judged, that a very small quantity of it, when disengaged from the sulphur with which it is naturally combined, would easily be distinguishable. Hjelm's process for disengaging the sulphur, by repeatedly burning linseed oil on the molybdæna in a crucible, and afterwards abstracting successive quantities of the same oil from it in a retort, was tried on a portion of the Sydney-Cove mineral, from which much of the white matter had been separated as above-mentioned. The black coal, remaining in the retort, became yellow by calcination, as that of molybdæna should do; but in this yellow powder, no vestige of molybdænic acid could be discovered.

Another quantity of the mineral was submitted to Scheele's own process, viz. repeated abstractions of diluted nitrous acid; but instead of becoming whiter every time, and at length white as chalk, which molybdæna should do, the blackness of this matter continued unaltered to the last. There is one circumstance in Mr. Scheele's experiments, which, though omitted by those who have given abstracts of them, may deserve, on the present occasion, to be more parti-

cularly noticed. He reduced the molybdæna into fine powder, and poured on it concentrated nitrous acid: "the mixture," he says, "was hardly lukewarm in the retort, when it passed all together into the recipient with great heat;" and it was for this reason that he afterwards used diluted acid. Presuming that this violent action of the concentrated nitrous acid might afford a decisive criterion of molybdæna, Mr. W. had the black residuum, after 5 or 6 abstractions of the diluted acid, ground fine on a levigating glass, and returned into the retort, with 6 times its weight of smoking spirit of nitre. The heat was increased cautiously far beyond lukewarm, but no commotion could be perceived, except the explosions already mentioned, which always took place when the mixture was near boiling. The distillation was continued to dryness, and repeated 5 times with smoking acid; but the mineral remained just as black as it was at first.

Now, as Scheele's molybdæna is slowly decomposed by the diluted nitrous acid, and rapidly acted on by the concentrated acid, while the black part of this mineral obstinately resists both, we cannot hesitate to conclude, that this black substance is not Scheele's molybdæna. There are some other circumstances which confirm this conclusion, though taken singly they would not perhaps be of much weight, considering the great proportion of other matter here mixed with the black. The principal of these circumstances are, that it yields no flowers before a blow-pipe, and that its particles seem to have no flexibility or elasticity, the only difficulty of reducing it into fine powder arising from a property of another kind, unctuosity. The difference, above noticed, between this black matter and common black-lead, consists only in the former leaving on calcination a white substance, seemingly siliceous, and the latter a brown ferruginous one. In their aspect, unctuosity, resistance to acids, and the volatility (in open fire) of that part in which the blackness consists, they perfectly agree; and they appear to agree also in the nature or constitution of this volatile part; for the Sydney-Cove mineral, as well as black-lead, deflagrates and effervesces very strongly with nitre, produces an hepatic impregnation on fusion with vitriolated alkali, but none with pure alkali, and is manifestly rich in inflammable matter, without sulphur.

It seems therefore, that this substance is a pure species of plumbago, or black-lead, not taken notice of by any writer. Fourcroy, in the last edition of his Chemistry, considers iron as an essential component part of black-lead, to which accordingly he gives a new name, expressive of that metal, carbure de fer. Lavoisier, in his Elements of Chemistry, lately published, mentions a carbure of zinc also, and says that both these carbures are called plumbago, or black-lead. The quantity of mineral Mr. W. had been furnished with was too far exhausted, before he met with this observation, to admit of any further experiments, for determining the presence of zinc in it; but those already stated, with the recollection of some circumstances attending them, persuade him, that that metallic

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body has no share in its composition. Neither before the blow-pipe, nor in calcination, was there any appearance of the peculiar flame, or flowers, by which zinc is so strongly characterized: if any such appearance had taken place, it could not have escaped notice, as some of the calcinations were particularly attended to during the process, though with a different view, the discovery of sulphur or arsenic. The white matter which remains after the calcination is certainly not calx of zinc, for it was not acted on by spirit of salt, cold or hot, while the calces of zinc are dissolved rapidly by that acid, even in the cold.

XVIII. Report on the Best Method of Proportioning the Excise on Spirituous Liquors. By Charles Blagden, M. D., Sec. R. S., and F. A. S. p. 321.

In consequence of an application from government to the president, Sir Joseph Banks, for the best means of ascertaining the just proportion of duty to be paid by any kind of spirituous liquor that should come before the officers of excise, Dr. B. was requested by that gentleman to assist in planning the proper experiments for this purpose, and to draw up the report on them when they should be finished.

Though various indications of the strength of spirituous liquors have been devised, applicable in a gross manner to general use, it is well known that no method admits of real accuracy but that of the specific gravity. The weights of an equal bulk of water and pure spirit differ from one another by at least a 6th part of the weight of the former; whence it is obvious, that when those two fluids are mixed together, the compound must have some intermediate specific gravity, approaching nearer to that of water or pure spirit, as the former or the latter is the more predominant ingredient. Were it not for a certain effect attending the mixture of water and spirits, which has been called their mutual penetration, the specific gravity of these compositions, in a given degree of heat, would be simply in the arithmetical proportion of the quantity of each of the fluids entering into them. But whenever different substances, which have a strong tendency to unite together, are mixed, the resulting compound is found to occupy less space than the substances forming it held in their separate state; therefore the specific gravity of such compounds is always greater than would be given by a simple calculation from the volume of their ingredients. Though it be a general fact, that such a decrease of bulk takes place on the mixture of substances which have a chemical attraction for each other, yet the quantity of this diminution is different in them all, and, under our present ignorance of the intimate composition of bodies, can be determined by experiment only. To ascertain therefore the quantity and law of the condensation, resulting from this mutual penetration of water and spirit, was the first object to which the following experiments were directed.

All bodies in general expand by heat; but the quantity of this expansion, as

well as the law of its progression, are probably not the same in any 2 substances. In water and spirit they are remarkably different. The whole expansion of pure spirit from 30° to 100° of Fahrenheit's thermometer, is not less than $\frac{1}{25}$th of its whole bulk at 30°; whereas that of water, in the same interval, is only $\frac{1}{145}$th of its bulk. The laws of their expansion are still more different than the quantities. If the expansion of quicksilver be, as usual, taken for the standard, the expansion of spirit is indeed progressively increasing with respect to that standard, but not much so within the above-mentioned interval; while water kept from freezing to 30°, which may easily be done, will absolutely contract as it is heated for 10° or more, that is, to 40° or 42° of the thermometer, and will then begin to expand as its heat is augmented, at first slowly, and afterwards gradually more rapidly, so as to observe on the whole a very increasing progression. Now mixtures of these 2 substances will, as may be supposed, approach to the less or the greater of those progressions, according as they are compounded of more spirit or more water, while their total expansion will be greater, according as more spirit enters into their composition; but the exact quantity of the expansion, as well as law of the progression, in all of them, can be determined only by trials. These were therefore the 2 other principal objects to be ascertained by experiment.

The first step towards a right performance of the experiments, was to procure the two substances, with which they were to be made, as pure as possible. Distilled water is in all cases so nearly alike, that no difficulty occurred with regard to it; but the specific gravity of pure spirit, or alcohol, has been given so very differently by the authors who have treated of it, that a particular set of experiments appeared necessary for determining to what degree of strength rectified spirits could conveniently be brought. The person engaged to make these experiments was Dr. Dollfuss, an ingenious Swiss gentleman then in London, who had distinguished himself by several publications on chemical subjects. Dr. Dollfuss, having been furnished by government with spirit for the purpose, rectified it by repeated and slow distillations, till its specific gravity became stationary in this manner of operating: he then added dry caustic alkali to it, let it stand for a few days, poured off the liquor, and distilled it with a small addition of burnt alum, placing the receiver in ice. By this method he obtained a spirit whose specific gravity was .8188 at 60° of heat. Perceiving however that he could not conveniently get the quantity of spirit he wanted lighter than .82527 at 60°, he fixed on that strength as a standard, to which he found the above-mentioned lighter spirit could be reduced by adding to it a $\frac{87}{1000}$th part of water; and with this spirit and distilled water he made a series of experiments for determining the specific gravity of different mixtures of these fluids in different degrees of heat.

The process followed by Dr. Dollfus is not here given as the best possible for obtaining pure spirit; nor was the result of it in fact the lightest alcohol that has been procured. Some spirit has been tried since that time, whose specific gravity

was .813 at 60°. This was furnished by Dr. George Fordyce, F. R. S., who succeeded in bringing it to that strength chiefly by adding the alkali very hot. Care must be taken that none of the caustic alkali comes over in the distillation. Some alcohol was also sent, for trial, by Mr. Lewis, an eminent distiller in Holborn, whose specific gravity, at the same temperature, was .814.

It was with spirit rectified from malt-spirits that Dr. Dollfuss's series of experiments was made; but he tried several comparative experiments with such as had been rectified from rum and brandy, and found no other difference than might fairly be ascribed to unavoidable errors. On examining the results of Dr. Dollfuss's experiments it was perceived, that though the numbers agreed together tolerably well on the whole, yet in some places there was that degree of irregularity in the first differences, as made it advisable to repeat several of the experiments; and Dr. Dollfus leaving England about that time, the business of this repetition was intrusted to Mr. Gilpin, clerk of the R. S. This gentleman had already taken a part in the business, by assisting Dr. Dollfuss in the former experiments, particularly in the very nice part of weighing the mixtures; and his great skill, accuracy, and patience, in conducting experiments, as well as in computations, had on other occasions been proved to many members of the society. One experiment leading on to another, Mr. Gilpin was at length induced to go through the whole series anew; and as the deductions in this report will be taken chiefly from this last set of experiments, it is proper here to describe minutely the method observed by Mr. Gilpin in his operation. This naturally resolves itself into 2 parts, the way of making the mixtures, and the way of ascertaining their specific gravity.

1. The mixtures were made by weight, as the only accurate method of fixing the proportions. In fluids of such very unequal expansions by heat as water and alcohol, if measures had been employed, increasing or decreasing in regular proportions to each other, the proportions of the masses would have been sensibly irregular; now the latter was the object in view, namely, to determine the real quantity of spirit in any given mixture, abstracting the consideration of its temperature. Besides, if the proportions had been taken by measure, a different mixture should have been made at every different degree of heat. But the principal consideration was, that with a very nice balance, such as was employed on this occasion, quantities can be determined to much greater exactness by weight, than by any practicable way of measurement. The proportions were therefore always taken by weight. A phial being provided of such a size as that it should be nearly full with the mixture, was made perfectly clean and dry, and being counterpoised, as much of the pure spirit as appeared necessary was poured into it. The weight of this spirit was then ascertained, and the weight of distilled water, required to make a mixture of the intended proportions, was calculated. This quantity of water was then added, with all the necessary care, the last

portions being put in by means of a well known instrument, which is composed of a small dish terminating in a tube drawn to a fine point: the top of the dish being covered with the thumb, the liquor in it is prevented from running out through the tube by the pressure of the atmosphere, but instantly begins to issue by drops, or a very small stream, on raising the thumb. Water being thus introduced into the phial, till it exactly counterpoised the weight, which, having been previously computed, was put into the opposite scale, the phial was shaken, and then well stopped with its glass stopple, over which leather was tied very tight, to prevent evaporation. No mixture was used till it had remained in the phial at least a month, for the full penetration to have taken place; and it was always well shaken before it was poured out to have its specific gravity tried.

2. There are 2 common methods of taking the specific gravity of fluids; one by finding the weight which a solid body loses by being immersed in them; the other by filling a convenient vessel with them, and ascertaining the increase of weight it acquires. In both cases a standard must have been previously taken, which is usually distilled water; namely, in the first method by finding the weight lost by the solid body in the water, and in the 2d method, the weight of the vessel filled with water. The latter was preferred for the following reasons. When a ball of glass, which is the properest kind of solid body, is weighed in any spirituous or watery fluid, the adhesion of the fluid occasions some inaccuracy, and renders the balance comparatively sluggish. To what degree this effect proceeds is uncertain; but from some experiments made by Mr. Gilpin, with that view, it appears to be very sensible. Also, in this method a large surface must be exposed to the air during the operation of weighing, which, especially in the higher temperatures, would give occasion to such an evaporation as to alter essentially the strength of the mixture. It seemed also, as if the temperature of the fluid under trial could be determined more exactly in the method of filling a vessel, than in the other: for the fluid cannot well be stirred while the ball to be weighed remains immersed in it; and as some time must necessarily be spent in the weighing, the change of heat which takes place during that period will be unequal through the mass, and may occasion a sensible error. It is true, on the other hand, that in the method of filling a vessel, the temperature could not be ascertained with the utmost precision, because the neck of the vessel employed, containing about 10 grains, was filled up to the mark with spirit not exactly of the same temperature, as will be explained presently; but this error, it is supposed, would by no means equal the other, and the utmost quantity of it may be estimated very nearly. Finally, it was much easier to bring the fluid to any given temperature when it was in a vessel to be weighed, than when it was to have a solid body weighed in it; because in the former case the quantity was smaller, and the vessel containing it more manageable, being readily heated with the hand or warm water, and cooled with cold water: the very circumstance, that

so much of the fluid was not required, proved a material convenience. The particular disadvantage in the method of weighing in a vessel, is the difficulty of filling it with extreme accuracy; but when the vessel is judiciously and neatly marked, the error of filling will, with due care, be exceedingly minute. By several repetitions of the same experiments, Mr. Gilpin seemed to bring it within the $\frac{1}{15000}$th part of the whole weight. The above-mentioned considerations induced the gentlemen employed in the experiments to give the preference to weighing the fluid itself; and that was accordingly the method practised both by Dr. Dollfuss and Mr. Gilpin in their operations.

The vessel chosen, as most convenient for the purpose, was a hollow glass ball, terminating in a neck of a small bore. That which Dr. Dollfuss used, held 5800 grains of distilled water; but, as the balance was so extremely accurate, it was thought expedient, on Mr. Gilpin's repetition of the experiments, to use one of only 2965 grains capacity, as admitting the heat of any fluid contained in it to be more nicely determined. The ball of this vessel, which may be called the weighing-bottle, measured about 2.8 inches in diameter, and was spherical, except a slight flattening on the part opposite to the neck, which served as a bottom for it to stand on. Its neck was formed of a portion of a barometer tube, .25 of an inch bore, and about $1\frac{1}{4}$ inch long; it was perfectly cylindrical, and on its outside, very near the middle of its length, a fine circle or ring was cut round it with a diamond, as the mark to which it was to be filled with the liquor. This mark was made by fixing the bottle in a lathe, and turning it round with great care, in contact with the diamond. The glass of this bottle was not very thick; it weighed 916 grains, and with its silver cap 936.

When the specific gravity of any liquor was to be taken by means of this bottle, the liquor was first brought nearly to the required temperature, and then the bottle was filled with it up to the beginning of the neck only, that there might be room for shaking it. A very fine and sensible thermometer was then passed through the neck of the bottle into the contained liquor, which showed whether it was above or below the intended temperature. In the former case the bottle was brought into colder air, or even plunged for a moment in cold water; the thermometer in the mean time being frequently put into the contained liquor, till it was found to sink to the right point. In like manner, when the liquor was too cold, the bottle was brought into warmer air, immersed in warm water, or more commonly held between the hands, till on repeated trials with the thermometer the just temperature was found. It will be understood, that during the course of this heating or cooling, the bottle was very frequently shaken between each immersion of the thermometer; and the top of the neck was kept covered, either with the finger, or a silver cap made on purpose, as constantly as possible. Hot water was used to raise the temperature only in heats of 80° and upwards, inferior heats being obtained by applying the hands to the bottle; when the hot

water was employed, the ball of the bottle was plunged into it, and again quickly lifted out, with the necessary shaking interposed, as often as was necessary for communicating the required heat to the liquor; but care was taken to wipe the bottle dry after each immersion, before it was shaken, lest any adhering moisture might by accident get into it. The liquor having by these means been brought to the desired temperature, the next operation was to fill up the bottle exactly to the mark on the neck, which was done with some of the same liquor, by means of a glass funnel with a very small bore. Mr. Gilpin endeavoured to get that portion of the liquor, which was employed for this purpose, pretty nearly to the temperature of the liquor contained in the bottle; but as the whole quantity to be added never exceeded 10 grains, a difference of 10° in the heat of that small quantity, which is more than it ever amounted to, would have occasioned an error of only $\frac{1}{30}$ of a degree in the temperature of the mass. Enough of the liquor was put in, to fill the neck rather above the mark, and the superfluous quantity was then absorbed to great nicety, by bringing into contact with it the fine point of a small roll of blotting paper. As the surface of the liquor in the neck would be always concave, the bottom or centre of this concavity was the part made to coincide with the mark round the glass: and in viewing it care was taken, that the near and opposite sides of the mark should appear exactly in the same line, by which means all parallax was avoided. A silver cap, which fitted tight, was then put upon the neck, to prevent evaporation; and the whole apparatus was in that state laid in the scale of the balance, to be weighed with all the exactness possible.

The spirit employed by Mr. Gilpin was furnished to him by Dr. Dollfuss, under whose inspection it had been rectified from rum supplied by Government. Its specific gravity, at 60° of heat, was .82514. It was first weighed pure, in the above-mentioned bottle, at every 5° of heat, from 30 to 100 inclusively. Then mixtures were formed of it and distilled water, in every proportion from $\frac{1}{10}$ of the water to equal parts of water and spirit; the quantity of water added being successively augmented, in the proportion of 5 grs. to 100 of the spirit; and these mixtures were also weighed in the bottle, like the pure spirit, at every 5° of heat. The numbers hence resulting are delivered in the following table; where the first column shows the degrees of heat; the 2d gives the weight of the pure spirit contained in the bottle at those different degrees; the 3d gives the weight of a mixture in the proportions of 100 parts by weight of that spirit to 5 of water, and so on successively till the water and the spirit are in equal parts. The bottle itself, with its cap, having been previously counterpoised, these numbers are the weights of the liquor contained in it, in grains and 100ths of a grain. They are the mean of 3 several experiments at least, as Mr. Gilpin always filled and weighed the bottle over again that number of times, if not oftener. The heat was taken at the even degree, as shown by the thermo-

meter, without any allowance in the first instance, because the coincidence of the mercury with a division can be perceived more accurately than any fraction can be estimated; and the errors of the thermometers, if any, it was supposed would be less on the grand divisions of 5°, than in any others. Mr. Gilpin used the same mixture throughout all the different temperatures, heating it up from 30° to 100°; hence some small error in its strength may have been occasioned, in the higher degrees, by more spirit evaporating than water; but this, it is believed, must have been trifling, and greater inconvenience would probably have resulted from interposing a fresh mixture.

TABLE I.

Weights at the different Degrees of Temperature.

Heat.	The pure spirit.	100 grs. of spirit to 5 grains of water.	100 grs. of spirit to 10 grains of water.	100 grs. of spirit to 15 grains of water.	100 grs. of spirit to 20 grains of water.	100 grs. of spirit to 25 grains of water.	100 grs. of spirit to 30 grains of water.	100 grs. of spirit to 35 grains of water.	100 grs. of spirit to 40 grains of water.	100 grs. of spirit to 45 grains of water.	100 grs. of spirit to 50 grains of water.
	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.
30°	2487.32	2519.98	2548.59	2573 86	2596.65	2617.24	2636.16	2653.54	2669.64	2684.63	2698.41
35	2480.79	2513.48	2541.96	2567.34	2590.15	2610.80	2629.77	2647.30	2663.48	2678.43	2692.32
40	2474.18	2506.98	2535.52	2560.83	2583.70	2604.50	2623.42	2641.02	2657.35	2672.37	2686.37
45	2467.52	2500.33	2528.90	2554.24	2577.16	2597.99	2617.04	2634.68	2650.96	2666.13	2680.25
50	2460.77	2493.48	2522.10	2547.61	2570.64	2591.50	2610.59	2628.26	2644.68	2659.95	2674.04
55	2453.84	2486.51	2515.30	2540.88	2563.94	2584.79	2604.07	2621.77	2638.25	2653.55	2667.72
60	2446.86	2479.75	2508.60	2534.19	2557.23	2578.22	2597.50	2615.26	2631.82	2647.20	2661.45
65	2440 04	2472.97	2501.87	2527.51	2550.56	2571.48	2590.86	2608.72	2625.41	2640.80	2655.09
70	2433.37	2466.28	2495.00	2530.65	2543.84	2564.89	2584.23	2602.14	2618.89	2634.30	2648.65
75	2426.47	2459.18	2488.03	2513.63	2536.91	2558.14	2577.47	2595.43	2612.20	2627.78	2642.17
80	2419.18	2451.95	2480.83	2506.61	2529.85	2551.10	2570.52	2588.61	2605.32	2621.03	2635.47
85	2412.02	2444.80	2473.68	2499.59	2523.08	2544.41	2563.80	2581.91	2598.76	2614.48	2628.87
90	2404.92	2437.72	2466.64	2492.62	2516.20	2537.57	2556.95	2575.20	2592.17	2607.86	2622.30
95	2397.75	2430.56	2459.51	2485 51	2509.15	2530.51	2549.95	2568.18	2585.12	2601.12	2615.70
100	2390.64	2423.53	2452.63	2478.59	2502.15	2523.59	2543.08	2561.28	2578.37	2591.45	2609.11

Heat.	100 grs. of spirit to 55 grains of water.	100 grs. of spirit to 60 grains of water.	100 grains of spirit to 65 grains of water.	100 grains of spirit to 70 grains of water.	100 grains of spirit to 75 grains of water.	100 grains of spirit to 80 grains of water.	100 grains of spirit to 85 grains of water.	100 grains of spirit to 90 grains of water.	100 grains of spirit to 95 grains of water.	100 grains of spirit to 100 grains of water.
	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.
30°	2711.19	2723 00	2733.84	2744.19	2753.67	2762.61	2771.26	2779.21	2786.47	2793.26
35	2705.08	2716.96	2727.78	2738.24	2747.90	2756.97	2765.47	2773.53	2780.75	2787.59
40	2699.09	2711.02	2721.90	2732.45	2742.18	2751.35	2759.85	2767.78	2775.15	2782.06
45	2692.97	2704.82	2715.98	2726.38	2736.21	2745.47	2754.13	2762.03	2769.55	2776.40
50	2686.81	2698.63	2709.92	2720.37	2730.27	2739.52	2748.22	2756.25	2763.67	2770.62
55	2680.57	2692.44	2703.67	2714.27	2724.20	2733.47	2742.25	2750.32	2757.82	2764.72
60	2674.31	2686.16	2697.44	2708.18	2718.26	2727.56	2736.26	2744.32	2751.87	2758.82
65	2667.97	2680.00	2691.22	2701.99	2712.06	2721.47	2730.27	2738.35	2745 93	2752.82
70	2661.67	2673 68	2685.02	2695.66	2705.87	2715.40	2724.22	2732.42	2740.00	2746.88
75	2655.19	2667.32	2678.60	2689.34	2699.57	2709.08	2717.95	2726.25	2733.92	2740.83
80	2648.47	2660.69	2672.02	2682.77	2693.03	2702.57	2711.50	2719.78	2727.49	2734.49
85	2641.85	2654.19	2665.54	2676.40	2686.77	2696.33	2705.37	2713.69	2721.47	2728.60
90	2635.38	2647.61	2659.07	2670.09	2680.60	2690.22	2699.10	2707.44	2715.22	2722.32
95	2628.83	2641.10	2652.56	2663.64	2674.20	2683.79	2692.81	2701.18	2708.91	2716.04
100	2622.22	2634.38	2645.95	2657.14	2667.61	2677.25	2686.36	2694.76	2702.50	2709.75

TABLE II.

Weights and Specific Gravities of Distilled Water.

Heat	Weight of the water.	Specific gravity of the water.
°	Grains.	
30		
35	2967.03	1.00087
40	2967.34	1.00091
45	2967.29	1.00084
50	2966.97	1.00066
55	2966.39	1.00040
60	2965.39	1.00000
65	2964.17	.99952
70	2962.72	.99896
75	2961.03	.99832
80	2959.13	.99762
85	2957.03	.99685
90	2954.80	.99602
95	2952.20	.99507
100	2949.36	.99404

In order to deduce the specific gravities from the numbers in the preceding table, it was necessary to weigh distilled water in the same vessel. This Mr. Gilpin did, in the same manner as before, at the different degrees of heat; and the result of his experiments is delivered in the annexed table, where the first column shows the heat, and the 2d gives the weight of the water, at that temperature, contained in the bottle.

There would be 2 methods of computing the specific gravity, at the different temperatures, from these numbers; one, by taking the weight of the water, at the particular temperature in question, for the standard; and the other by fixing on one certain temperature of the water, for instance 60°, to be the standard, with its bulk at which that of the spirit at all different degrees shall be compared. The latter method was preferred, though not the most usual, because it shows, more readily and simply, the progression observed in the changes of specific gravity, according to the heat and strength of the mixture. This method however rendered it necessary to make an allowance for the contraction and expansion of the bottle used for weighing the liquors, according to the deviation of their temperature from 60°, either below or above. To obtain this correction, the expansion of hollow glass was taken from General Roy's experiments in the 75th volume of the Philos. Trans. as .0000517 of an inch on a foot for every degree of heat; whence its effect, in enlarging the capacity of a sphere, was computed, and the resulting correction added to the weight of the liquors in heats below 60°, and subtracted from it in heats above. On the same account a 3d column is given, in the preceding table, to show the specific gravity of water at the different temperatures, its weight at 60° being taken as the standard.

Another correction also became necessary, on account of the part of the stem of the thermometer which was not immersed in the liquor. This instrument made by Ramsden, had its ball .22 of an inch in diameter, and its stem 13 inches in length. From the ball to the commencement of the scale 3.6 inches of the stem were bare, and then the scale began, which reached from 15 to 110°. The part of it particularly used in these experiments, namely, from 30° to 100°, measured 6.82 inches. The scale was made of ivory, and carried divisions to every 5th of a degree, the quarters of which could be readily estimated; so that the instrument could be read off to 20ths of degrees. When the thermometer was immersed in the weighing-bottle, the liquor reached up

nearly to what would have been 0° on its stem; hence, as the heat of the room in which the experiments were made remained about 60°, the correction for the different heat of the quicksilver in the stem from that in the ball of the thermometer was calculated according to Mr. Cavendish's table, given in the 67th volume of the Philos. Trans. Thus the real heat of the fluid in the weighing-bottle being found, an allowance was made to reduce it to the exact degree indicated on the scale of the thermometer. The precise specific gravity of the pure spirit employed was .82514; but to avoid an inconvenient fraction it is taken, in constructing the table of specific gravities, as .825 only, a proportionable deduction being made from all the other numbers. Thus the following table gives the true specific gravity, at the different degrees of heat, of a pure rectified spirit, whose specific gravity at 60° is .825, together with the specific gravities of different mixtures of it with water, at those different temperatures, as far as equal parts by weight.

TABLE III.

Real Specific Gravities at the different Temperatures.

Heat.	The pure spirit.	100 grs. of spirit to 5 grs. of water.	100 grs. of spirit to 10 grs. of water	100 grs. of spirit to 15 grs. of water.	100 grs. of spirit to 20 grs. of water.	100 grs. of spirit to 25 grs. of water.	100 grs of spirit to 80 grs of water.	100 grs. of spirit to 35 grs. of water.	100 grs. of spirit to 40 grs. of water.	100 grs. of spirit to 45 grs. of water.	100 grs. of spirit to 50 grs. of water.	100 grs. of spirit to 55 grs. of water.
30°	.83899	.85001	.85967	.86819	.87589	.88284	.88922	.89509	.90053	.90558	.91023	.91454
35	.83673	.84776	.85737	.86592	.87363	.88061	.88700	.89292	.89839	.90343	.90811	.91242
40	.83445	.84551	.85515	.86367	.87140	.87843	.88481	.89074	.89626	.90133	.90605	.91034
45	.83215	.84321	.85286	.86140	.86913	.87617	.88260	.88855	.89405	.89916	.90393	.90822
50	.82981	.84084	.85051	.85910	.86688	.87392	.88036	.88632	.89187	.89702	.90177	.90608
55	.82741	.83843	.84815	.85677	.86455	.87159	.87809	.88406	.88963	.89479	.89957	.90390
60	.82500	.83609	.84583	.85445	.86223	.86931	.87582	.88181	.88740	.89259	.89741	.90173
65	.82262	.83374	.84350	.85213	.85991	.86698	.87352	.87954	.88518	.89037	.89518	.89952
70	.82032	.83142	.84111	.84975	.85758	.86469	.87121	.87725	.88291	.88810	.89294	.89733
75	.81792	.82896	.83869	.84731	.85517	.86234	.86886	.87491	.88058	.88585	.89068	.89507
80	.81543	.82649	.83623	.84491	.85276	.85993	.86648	.87258	.87822	.88352	.88839	.89277
85	.81291	.82396	.83371	.84243	.85036	.85757	.86411	.87021	.87590	.88120	.88605	.89043
90	.81044	.82150	.83126	.84001	.84797	.85518	.86172	.86787	.87360	.87889	.88376	.88817
95	.80794	.81900	.82877	.83753	.84550	.85272	.85928	.86542	.87114	.87654	.88146	.88588
100	.80548	.81657	.82639	.83513	.84308	.85031	.85688	.86302	.86879	.87421	.87915	.88357

Heat.	100 grs. of spirit to 60 grs. of water.	100 grs. of spirit to 65 grs. of water.	100 grs. of spirit to 70 grs. of water.	100 grs. of spirit to 75 grs. of water.	100 grs. of spirit to 80 grs. of water.	100 grs. of spirit to 85 grs. of water.	100 grs. of spirit to 90 grs. of water.	100 grs. of spirit to 95 grs. of water.	100 grs. of spirit to 100 grs. of water.
30°	.91853	.92219	.92568	.92888	.93191	.93483	.93751	.93996	.94225
35	.91644	.92009	.92362	.92687	.92995	.93281	.93553	.93796	.94027
40	.91438	.91805	.92161	.92489	.92799	.93086	.93353	.93602	.93835
45	.91222	.91599	.91950	.92281	.92595	.92887	.93153	.93407	.93638
50	.91007	.91388	.91740	.92075	.92388	.92681	.92952	.93202	.93436
55	.90791	.91170	.91528	.91863	.92176	.92472	.92744	.92997	.93230
60	.90570	.90954	.91316	.91656	.91971	.92264	.92536	.92791	.93025
65	.90359	.90738	.91100	.91440	.91769	.92055	.92328	.92584	.92816
70	.90136	.90522	.90880	.91225	.91547	.91845	.92121	.92377	.92608
75	.89916	.90298	.90660	.91005	.91326	.91625	.91909	.92164	.92397
80	.89690	.90072	.90435	.90780	.91103	.91404	.91683	.91943	.92179
85	.89460	.89843	.90209	.90558	.90882	.91186	.91465	.91729	.91969
90	.89230	.89617	.89988	.90342	.90668	.90967	.91248	.91511	.91751
95	.89003	.89390	.89763	.90119	.90443	.90747	.91029	.91290	.91531
100	.88769	.89158	.89536	.89889	.90215	.90522	.90805	.91066	.91310

From this table, when the specific gravity of any spirituous liquor is ascertained, it will be easy to find the quantity of rectified spirit, of the above-mentioned standard, contained in any given quantity of it, either by weight or measure. As common arithmetic is competent to furnish the rules for this purpose, it would be superfluous to give them here. All the objects of inquiry relative to this business should, Dr. B. thinks, be reduced to tables; the first of which might exhibit the specific gravities of different mixtures, from 1 to 100 parts of water, increasing by 1, at every degree of heat from 40 to 80, being the utmost limits of temperature that can be wanted in common practice. This table need only be calculated to 3 places of figures, which will always give the quantity of spirit true within a 50th part of the whole, and in the most usual degrees of heat within a 100th; and to this number of figures the areometer, or hydrometer, showing the specific gravities, could be suited. A further reason for continuing only to 3 places of figures is, that, accurate as Mr. Gilpin's experiments have been, some irregularities are found in the last 2 of the 5 decimals to which his tables are calculated. The greatest of these irregularities do not exceed the quantity corresponding to a difference of $\frac{1}{10}$ of a degree of heat, and in general they are much less. A table might be constructed to show what the numbers would probably have been, to the 5 places of decimals, if there had been no kind of error in the experiments.—Another table should be of the volumes, exhibiting what proportion the spirit and water bore to each other by measure or bulk, in the different mixtures; whence might be calculated a very useful table of diminutions, to show when a given weight, or volume, of a certain spirit and water are mixed together, how much their bulk would be diminished; or, what is called by the distillers the concentration. From such a table the distiller could learn what quantity of water he must mix with spirit of a given strength, in order to reduce it to proof spirit, or any other strength; and also what quantity of proof spirit, or spirit of any other strength, he may obtain, by adding water to spirit of a given strength; both circumstances very necessary to be known in the trade, and which some of the sliding rulers now in use profess to point out.

It may appear odd, that no mention has been made till now of proof spirit, the standard to which most of the regulations of the excise have hitherto been referred. The reasons for not adopting this standard are: first, that the strength of spirit to be called proof is a mere arbitrary point, and by no means so exactly determined as could be wished; and 2dly, that it seemed most convenient to take for the standard the highest strength of spirit usually found in commerce, and beyond which it cannot be rectified without a process of some expense, so that all the other degrees of strength might be reckoned one way, without the intervention of a middle point, inducing the necessity of denomi-

nating some above and others under. If however Government should find it expedient to preserve the reference to proof spirit, from the tables given in this report others may be constructed, in which all the old terms of over and under proof should be retained, and have a precise meaning, as soon as the strength to be called proof shall be finally settled. By the Act of 2 Geo. III. it is ordered, that the gallon of brandy or spirits of the strength of 1 to 6 under proof, shall be taken and reckoned at 7 lb. 13 oz. which is understood by the trade to mean at 55° of heat. Hence, taking the weight of a gallon of water at the same heat to be 8 lb. 5.66 &c. oz.,* the specific gravity of this diluted spirit will be found .9335 at 60;† whence, by a computation founded on the tables in this report, the specific gravity of proof spirit will come out .916. But the rulers of correction belonging to Dicas's and Quin's hydrometers give the specific gravity of proof spirits about .922 at 55°, equivalent to .920 at 60°. The former, .916, corresponds to a mixture of 100 parts of spirit with 62 by measure, or 75 by weight, of water; and the latter, .920, to a mixture of 100 parts of spirit and 66 by measure, or 80 by weight, of water. The difference is considerable; but the first is undoubtedly most conformable to the existing acts of parliament. If therefore it be thought right to preserve the term proof-spirit in our excise laws, it may be understoood to mean spirit, whose specific gravity is .916, and which is composed of 100 parts of rectified spirit at .825, and 62 parts of water by measure, or 75 by weight; the whole at 60 degrees of heat.

Dr. B. has chosen this point of the thermometer, 60°, in preference to 55°, because it is much the most suitable for experiments, being the temperature at which a room feels pleasant, and in which any operation, however slow and tedious, can be executed without the uneasy sensation of cold: for this reason it has been adopted by many English philosophers. In the table formerly recommended, from 40 to 80 degrees of the thermometer, it will be the middle temperature.

The specific gravity of .825 having been fixed on as the standard of rectified spirit in our tables, Mr. Gilpin was desired to ascertain by experiment what proportion of water would be necessary, to reduce the lightest alcohol in his possession to that standard. This was some of the alcohol which Mr. Lewis had furnished; and its specific gravity being .814196 at 60°, 3000 grains of it mixed with 135 grains of distilled water formed a compound, whose specific gravity was .825153; that is, in round numbers, 100 grains of alcohol at .814 with 4.5 grains of water, form our standard of spirit at .825.

* Probably 8 lb. 5.72 oz. is nearer.—Orig.

† This specific gravity indicates a mixture of 107 grains of water with 100 of spirit, and consequently is below Mr. Gilpin's present tables, which go only to equal parts.—Orig.

On Hydrometers.—The readiest way of ascertaining specific gravities, and doubtless the most convenient for public business, is by hydrometers; and those of the simplest construction must be best on the whole, especially if more accurate means are kept at hand, to be resorted to in case of disputes. An hydrometer of glass would be the most certain; but whether it be of that substance, or of metal, it should consist of a ball, or rather bulb, so poised as that a certain part should be always downmost in the liquor, and having a stem rising from it on the opposite part, which would consequently keep upright in using the instrument. On the size of this stem, the sensibility of the hydrometer chiefly depends. In the old areometers the stem was made so large, that the volume of water displaced between its least and greatest immersions was equal to the whole difference of specific gravity between water and alcohol, or perhaps more; whence its scale of divisions must be very small, and could not give the specific gravity with much accuracy. To remedy this defect, weights were introduced, by means of which the stem could be made smaller, each weight affording a new commencement of its scale; so that the size of the divisions on a given length of stem was doubled, tripled, quadrupled, &c. according as 1, 2, 3, or more weights were employed, the diameter of the stem being lessened in the subduplicate proportion of the increased length of the divisions. Of late this principle seems to have been carried to excess; the number of weights adapted to some hydrometers being so great as to prove very inconvenient in practice. A mean between the 2 methods would certainly be best, which might be suited to our tables in the following manner.

It is proposed to determine the specific gravity to 3 places of decimals, water being taken as unity: the whole compass of numbers therefore, from rectified spirit of water, at 60° of heat, would be the difference between .825 and 1.000, that is, 175; call it 220 to include the lightest spirit and heaviest water, at all the common temperatures. Of these divisions the stem might give every 20, and then 10 weights would be sufficient for the whole 220. By making the stem carry 20 divisions, an inconvenience much complained of, that of shifting the weights, would in great measure be avoided; because a person conversant in such business would seldom err to that extent in judging of the strength of his spirit previous to trial; and yet the stem would not need to be so large, or the divisions so small as to preclude the desired accuracy. In conformity to this arrangement it would be proper, that the weights adapted to the hydrometer should be marked with the numbers of the specific gravity, zero on the top of its stem, without a weight, being supposed to mean 800, and 20 at the bottom of the stem to signify 820, which number the first weight would carry; the successive weights would be marked 840, 860, &c.; and the division on the stem cut by the fluid under trial would be a number to be always added to the number marked on the

weight, the sum of the two showing the true specific gravity. The weights should unquestionably be made to apply on the top of the stem, so as never to come into contact with the liquor; and in using the hydrometer, its stem should always be pressed down lower than the point at which it will ultimately rest, that by being wetted it may occasion no resistance to the fluid. The instrument itself should be of as regular a shape, and with as few inequalities and protuberances, as possible, that all unnecessary obstruction to its motions may be avoided.

As it is not probable but disputes will sometimes arise, it would be advisable, that some of the principal excise offices should be provided with a good pair of scales, and a weighing-bottle properly marked, the quantity of whose contents of distilled water at 60° had been previously determined. By filling this bottle up to the mark with the spirit in question, and dividing its increase of weight by the given weight of water required to fill it, the specific gravity of the spirit might be better ascertained, even under the management of a common operator, than by the most dexterous use of the hydrometer.

The simplest and most equitable method of levying the duty on spirituous liquors would be, to consider rectified spirit as the true and only excisable matter. On this principle, all such liquors would pay exactly according to the quantity of rectified spirit they contain; so that when a cask, for instance, of any spirits was presented to the revenue officer, his business would be to determine from the quantity, specific gravity, and temperature, of the liquor, how many gallons, or pounds, of rectified spirit enter into its composition; each of which gallons, or pounds, should be charged a certain sum. The complicated regulations attending the adaption of the duties to different degrees of strength would thus be avoided; and it is believed that many frauds might be prevented, which artful persons have now an opportunity of practising, by altering the strength of their spirit in a variety of ways. From the tables already recommended, it would be easy to deduce this quantity of rectified spirit, either by weight or measure, in any given quantity of a spirituous liquor; or other tables might be constructed which should show it at once by inspection.

If however it be thought by government most expedient not to make any essential change in the present manner of collecting this article of the revenue, Dr. B. would at least recommend, that the specific gravity should be substituted for the relation to proof spirit. Thus, instead of ordering so much duty per gallon to be paid by spirits 1 to 6 under proof, it may be enacted, that the same sum shall be paid by spirit of .9335 specific gravity, or, not to be too precise, by spirit from .930 to .935, and so on for any other degrees of strength; a certain temperature, suppose 60°, being always understood to be meant when specific gravity is mentioned in an Act of Parliament. The duties to be laid according to either of these methods may readily be adjusted or equalized to those paid at

present, as far as the latter can be determined from the act of 2 George III. referred to above, or by any of the instruments now in use.

XIX. Observations on the Sugar Ants. By John Castles, Esq.* p. 346.

The sugar ants, so called from their ruinous effects on the sugar-cane, first made their appearance in Grenada about the year 1770, on a sugar plantation at Petit Havre, a bay 5 or 6 miles from the town of St. George, the capital, conveniently situated for smuggling from Martinique. It was therefore concluded, they were brought from thence in some vessel employed in that trade; which is very probable, as colonies of them in like manner were afterwards propagated in different parts of the island by droghers, or vessels employed in carrying stores, &c. from one part of the island to another. Thence they continued to extend themselves on all sides, for several years; destroying in succession every sugar plantation between St. George's and St. John's, a space of about 12 miles. At the same time colonies of them began to be observed in different parts of the island, particularly at Duquesne on the north, and Calavini on the south side of it.

All attempts of the planters to put a stop to the ravages of these insects having been found ineffectual, it well became the legislature to offer great public rewards for discovering a practicable method of destroying them, so as to permit the cultivation of the sugar-cane as formerly. Accordingly, an act was passed, by which such discoverer was entitled to 20,000 pounds, to be paid from the public treasury of the island. Many were the candidates on this occasion, but very far were any of them from having any just claim: yet considerable sums of money were granted, in consideration of trouble and expenses in making experiments. In Grenada there had always been several species of ants, differing in size, colour, &c. which however were perfectly innocent with respect to the sugar-cane. The ants in question, on the contrary, were not only highly injurious to it, but to several sorts of trees, such as the lime, lemon, orange, &c.

These ants are of the middle size, of a slender make, of a dark red colour, and remarkable for the quickness of their motions; but their greatest peculiarities were, their taste when applied to the tongue, the immensity of their number, and their choice of places for their nests. All the other species of ants in Grenada have a bitter musky taste. These, on the contrary, are acid in the highest degree, and, when a number of them were rubbed together between the palms of the hands, they emitted a strong vitriolic sulphureous smell; so much so, that when this experiment was made, a gentleman conceived that it might be owing to this quality that these insects were so unfriendly to vegetation. This criterion

* This species of ant is perhaps the *Formica saccharivora* of the Gmelinian edition of the Systema Naturæ.

to distinguish them was infallible, and known to every one. Their numbers were incredible. The roads are seen coloured by them for miles together: and so crouded were they in many places, that the print of the horses feet would appear for a moment or two, till filled up by the surrounding multitude. All the other species of ants, though numerous, were circumscribed and confined to a small spot, in proportion to the space occupied by the cane ants, as a mole hill to a mountain. The common black ants of that country had their nests about the foundation of houses or old walls; others in hollow trees; and a large species in the pastures, descending by a small aperture under ground. The sugar ants universally constructed their nests among the roots of particular plants and trees, such as the sugar-cane, lime, lemon, and orange trees, &c.

The destruction of these ants was attempted chiefly by poison and the application of fire. For the first purpose, arsenic and corrosive sublimate, mixed with animal substances, such as salt fish, herrings, crabs, and other shell-fish, &c. were used, which was greedily devoured by them. Myriads of them were thus destroyed; and the more so, as it was observed by a magnifying glass, and indeed by the naked eye, that corrosive sublimate had the effect of rendering them so outrageous that they destroyed each other; and that effect was produced even by coming into contact with it. But it is clear, and it was found, that these poisons could not be laid in sufficient quantities over so large a tract of land, as to give the hundred-thousandth part of them a taste, and consequently they proved inadequate to the task.

The use of fire afforded a greater probability of success; for it was observed, that if wood, burnt to the state of charcoal, without flame, and immediately taken from the fire, was laid in their way, they crouded to it in such amazing numbers as soon to extinguish it, though with the destruction of thousands of them in effecting it. This part of their history appears scarcely credible; but, on making the experiment himself, Mr. C. found it literally true. He laid fire, as above described, where there appeared but a very few ants, and in the course of a few minutes thousands were seen crouding to it and on it, till it was perfectly covered by their dead bodies. Holes were therefore dug at proper distances in a cane piece, and fire made in each of them. Prodigious quantities perished in this way; for those fires, when extinguished, appeared in the shape of mole-hills, from the numbers of their dead bodies heaped on them. Yet they soon appeared again as numerous as ever. This may be accounted for, not only from their amazing fecundity, but that probably none of the breeding ants, or young brood, suffered from the experiment. For the same reason, the momentary general application of fire by burning the cane trash, or straw of the cane, as it lay on the ground, proved as little effectual; for though perhaps multitudes of ants might have been destroyed, yet in general they would escape by retiring to

their nests under cover, and out of its reach, and the breeding ants, with their young progeny, must have remained unhurt.

Mr. Smeathman, who wrote a paper on the termites, or white ants, of Africa, and was at Grenada at this time, imagined that these ants were not the cause of the injury done to the canes. He supposed it was owing to the blast, a disease the canes are subject to, said to arise from a species of small flies, generated on their stems and leaves; and that the ants were attracted in such multitudes merely to feed on them. There is no doubt, that where this blast existed, it constituted part of the food of the ants: but this theory was overthrown, by observing, that by far the greatest part of the injured canes had no appearance of that sort, but became sickly and withered, apparently for want of nourishment. Besides, had that been the case, the canes must have been benefited instead of being hurt by these insects. For the cure of the blast, he proposed the application of train oil, which had not the least effect in preventing the mischief, and, if it had, could never have been generally enough used to answer the purpose.

This calamity, which resisted so long the efforts of the planters, was at length removed by another, which, however ruinous to the other islands in the West Indies, and in other respects, was to Grenada a very great blessing, namely, the hurricane in 1780; without which it is probable the cultivation of the sugar-cane in the most valuable parts of that island must have in a great measure been thrown aside, at least for some years. How this hurricane produced this effect has been considered rather as a matter of wonder and surprise than attempted to be explained. By attending to the following observations, the difficulty will probably be removed.

These ants make their nests, or cells for the reception of their eggs, only under or among the roots of such trees or plants as are not only capable of protecting them from heavy rains, but are at the same time so firm in the ground as to afford a secure basis to support them against any injury occasioned by the agitation of the usual winds. This double qualification the sugar-cane possesses in a very great degree; for a stool of canes, which is the assemblage of its numerous roots where the stems begin to shoot out, is almost impenetrable to rain, and is also, from the amazing numbers and extension of the roots, firmly fixed to the ground. Thus, when every other part of the field is drenched with rain, the ground under those stools will be found quite dry, hence, in ordinary weather, their nests are in a state of perfect security. The lime, lemon, orange, and some other trees, afford these insects the same advantages, from the great number and quality of their roots, which are firmly fixed to the earth, and are very large; besides which, their tops are so very thick and umbrageous as to prevent even a very heavy rain from reaching the ground underneath.

On the contrary, these ants' nests are never found at the roots of trees or

plants incapable of affording the above protection: such, for instance, is the coffee tree. It is indeed sufficiently firm in the ground, but it has only one large tap root, which goes straight downwards, and its lateral roots are so small as to afford no shelter against rain. So again, the roots of the cotton shrub run too near the surface of the earth to prevent the access of rain, and are neither sufficiently permanent, nor firm enough to resist the agitation by the usual winds. The same observation will be found true with respect to cocoa, plantains, maize, tobacco, indigo, and many other species of trees and plants. Trees or plants of the first description always suffer more or less in lands infested with these ants; whereas those of the latter never do. Hence we may fairly conclude, that the mischief done by these insects is occasioned only by their lodging and making their nests about the roots of particular trees or plants. Thus the roots of the sugar-canes are somehow or other so much injured by them, as to be incapable of performing their office of supplying due nourishment to the plants, which therefore become sickly and stinted, and consequently do not afford juices fit for making sugar in either tolerable quantity or quality.

That these ants do not feed on any part of the canes or trees affected, seems very clear, for no loss of substance in either the one or the other has ever been observed; nor have they ever been seen carrying off vegetable substances of any sort. The truth of this will further appear by the following fact. A very fine lime-tree, in the pasture of Mount William estate, at a considerable distance from any canes, but near the dwelling house, had sickened and died soon after the ants made their appearance on that estate. After it had remained in that state, without a single leaf, or the least verdure, for several months, on examination, a very few ants appeared about it; but when with the manager's permission it was grubbed out, a most astonishing quantity of ants and ants' nests, full of eggs, were found about its roots, all of which were quite dead, and many of them rotten. That this tree constituted no part of their food is quite certain; but, while it continued to afford them proper security for their nests, they still continued their abode.

On the contrary, there is the greatest presumption that these ants are carnivorous, and feed entirely on animal substances; for if a dead insect, or animal food of any sort, was laid in their way, it was immediately carried off. It was found almost impossible to preserve cold victuals from them. The largest carcases, as soon as they began to become putrid, so as that they could separate the parts, soon disappeared. Negroes with sores had difficulty to keep the ants from the edges of them. They destroyed all other vermin, rats in particular, of which they cleared every plantation they came on, which they probably effected by attacking their young. It was found that poultry, or other small stock, could be raised with the greatest difficulty; and the eyes, nose, and other emunctories

of the bodies of dying or dead animals, were instantly covered with them. In the year 1780, many of the sugar estates which had been first infested with these ants had been either abandoned, or put into other kinds of produce, principally cotton; which, as above observed, do not afford conveniency for their nests. In consequence, the ants had there so much decreased in number, that the cultivation of sugar had again begun to be re-assumed. But it was very different in those plantations which had but lately been attacked, and were still in sugar. At Duquesne particularly at that time they were pernicious in the highest degree, spreading themselves on all sides with great rapidity, when a sudden stop was put to their progress by the hurricane which happened near the middle of October that year. How this was effected may be explained by attending to the above observations.

From what has been said it appears, that a dry situation, so as to exclude the ordinary rains from their nests or cells, appropriated for the reception of their eggs or young brood, is absolutely necessary; but that these situations, however well calculated for the usual weather, could not afford this protection from rain during the hurricane, may be easily conceived. When by the violence of the tempest heavy pieces of artillery were removed from their places, and houses and sugar-works levelled with the ground, there can be no doubt that trees and every thing growing above ground must have greatly suffered. This was the case. Great numbers of trees and plants, which resist commonly the ordinary winds, were torn out by the root. The canes were universally either lodged or twisted about as if by a whirlwind, or torn out of the ground altogether. In the latter case, the breeding ants, with their progeny, must have been exposed to inevitable destruction from the deluge of rain which fell at the same time. The number of canes however, thus torn out of the ground, could not have been adequate to the sudden diminution of the sugar ants; but it is easy to conceive that the roots of canes which remained on the ground, and the earth about them, were so agitated and shaken, and at the same time the ants' nests were so broken open, or injured by the violence of the wind, as to admit the torrents of rain accompanying it. Probably therefore the principal destruction of these ants must have been thus effected.

Two circumstances tended to facilitate this happy effect. Many of the roots of the canes infected, as above observed, were either dead or rotten, so as not to be capable of making the same resistance to the wind as those in perfect health. And this hurricane happened so very late as the month of October, when the canes are always so high above ground as to give the wind sufficient hold of them, which at an earlier period would not have been the case. That many of the cane ants were swept off by the torrents of rain into the rivers and ravines, and thus perished, cannot be doubted; but if we consider the obstacles to this being very

general, it could have had but small effect in considerably reducing their numbers; for on flat land it could not have happened. In hanging or hilly land, the cane trash would afford great shelter, and the ants would naturally retire to their nests for security, when they found their danger.

Some have supposed, that the sugar ants, after a certain time, degenerate, and become inoffensive; and in proof of this, they say, Martinique and Barbadoes were freed from their bad effects without a hurricane or any other apparent cause. The idea of any such extraordinary and unheard-of deviation of nature, is too contemptible to deserve an answer; but the reason is obvious. The planters there either abandoned their cane lands, or planted them in coffee, cocoa, cotton, indigo, &c. none of which, according to the above observations, afford the ants proper conveniency for the propagation of their species; and therefore their numbers must have so much decreased as to re-admit the culture of the sugar-cane as before. At the same time it is very probable, that this diminution might have in part been owing to something of the hurricane kind; for it is well known that strong squalls of wind, attended with heavy rains, are frequent in the West Indies, though they do not last so long, nor are so violent, as to deserve the name of a hurricane.

All that has been said on this subject would certainly be of little or no consequence, did it not lead to the true method of cultivating the sugar-cane on lands infested with those destructive insects; in which point of view however it becomes important. If then the above doctrine be just, it follows that the whole of our attention must be turned to the destruction of the nests of these ants, and consequently the breeding ants with their eggs or young brood. In order to effect this, all trees * and fences, under the roots of which these ants commonly take their residence, should first be grubbed out: particularly lime fences, which are very common in Grenada, and which generally suffered from the ants before the canes appeared in the least injured. After which the canes should be stumped out with care, and the stools burnt as soon as possible, together with the field trash, or the dried leaves and tops of the canes, to prevent the ants from making their escape to new quarters. The best way of doing this would probably be, to gather the field trash together in considerable heaps, and to throw the stools as soon as dug out of the ground into them, and immediately apply fire. By this means multitudes must be destroyed; for the field trash, when dry, burns with great rapidity. The land should then be ploughed or hoe-ploughed twice, or once at least, in the wettest season of the year, to admit the rains, before it is hoed for planting the cane: by these means these insects might be so much reduced in number as at least to secure a good plant cane.

* Particular fruit trees may probably be preserved, without detriment, by carefully removing the earth from about their roots, destroying the ants' nests, and afterwards replacing either the same or new earth.—Orig.

But it is the custom in most of the West India islands to permit the canes to rattoon; that is, after the canes have once been cut down, for the purpose of making sugar, they are suffered to grow up again, without replanting; and this generally for 3 or 4 years, but sometimes for 10, 15, or 20. In this mode of culture the stools become larger every year, so as to grow out of the ground to a considerable height, and by that means afford more and more shelter to the ants' nests; therefore, for 2 or 3 successive crops, the canes should be replanted yearly, so as not only to afford as little cover as possible for the ants' nests, but continually to disturb such ants as may have escaped, in the business of propagating their species.

That considerable expense and labour will attend putting this method into execution, cannot be doubted. An expensive cure however is better than none; but from the general principles of agriculture, Mr. C. is of opinion, that the planter will be amply repaid for his trouble, by the goodness of his crops, in consequence of the superior tilth the land will receive in the proposed method. Of this we have a proof in the island of St. Kitt's, where they constantly replant their canes yearly; and it is very well known, that an acre of cane land there gives a greater return than the same quantity in any other island. In St. Kitt's, 5 hogsheads per acre is common yielding in good land. In Grenada, from 2 to 3 hogsheads from plant canes, and half that quantity from rattoons. Thus, though the St. Kitt's planter cuts only one half his cane land yearly, in a given number of years he makes a greater revenue than the Grenada planter on the present mode of rattooning, when $\frac{1}{4}$ of the cane land is yearly cut.

Some may be of opinion, that it would be more advantageous to change the produce than to pursue the proposed method; on which Mr. C. observes, that it appears $\frac{1}{4}$ of the usual crop of sugar, thus produced, will be more advantageous to the planter, when at the same time progress is making in destroying the sugar ants, than a full crop of any other produce. In some very few situations cotton perhaps may be excepted. As to coffee, it is to be considered that it gives no return till the 3d year after planting, and not a full crop till the 5th. Cocoa begins to bear in 5 years; but yields little till the 7th; and indigo not only exceedingly impoverishes the land, but is unhealthy to the negroes. Add to this, that far the greatest part of sugar lands are unfit for the culture of any of these.

XX. Experiments and Observations on the Dissolution of Metals in Acids, and their Precipitations; with an Account of a New Compound Acid Menstruum, useful in some Technical Operations of Parting Metals. By James Keir, Esq., F. R. S. p. 359.

In the following paper, says Mr. K., I intend to relate 2 sets of experiments; one, showing the effects of compounding the vitriolic and nitrous acids in dis-

solving metals; and the other, describing some curious appearances which occur in the precipitation of silver from its solution in nitrous acid by iron, and by some other substances. In a subsequent paper I hope to continue the subject of metallic dissolution* and precipitation, first, by adding some experiments on the quantities and kinds of gas produced by dissolving different metals in different acids, under various circumstances; 2dly, by submitting certain general propositions, which seem deducible from the facts related; and lastly, by concluding with some reflections relative to the theory of metallic dissolution and precipitation.

PART 1. *On the effects of compounding the vitriolic and nitrous acids, under various circumstances, on the dissolution of metals.*

§ 1. *On the mixture of oil of vitriol and nitre.*—1. The properties of the several acids, in their separate states, have been investigated with considerable industry and success; and those of one compound, aqua regis, are well known, on account of its frequent use in dissolving gold: yet not only various other combinations of different acids remain to be examined; but also the changes of properties to which these mixed acids are subject, from the difference of circumstances, especially those of concentration, temperature, and of that quality which is called, properly or improperly, phlogistication, are subjects still open for inquiry.

2. As I shall have frequent occasion to speak of the phlogistication and dephlogistication of acids, I wish to premise, that by these terms I mean only certain states or qualities of those bodies, but without any theoretical reference. Thus vitriolic acid may be said to be phlogisticated by addition of sulphur or other inflammable matter, by which it is converted into sulphureous acid, without determining whether this change be caused by the addition of the supposed principle phlogiston, as one set of philosophers believe, or by the action of the added inflammable substance in drawing from the acid a portion of its aërial principle, by which the sulphur, its other element, is made to predominate, as others have lately maintained. It were much to be wished that we had words totally unconnected with theory; that chemists, who differ from each other in some speculative points, may yet speak the same language, and may relate their facts and observations, without having our attention continually drawn aside from these, to the different modes of explanation which have been imagined. But at present

* The English word solution has two significations in chemistry; one, expressive of the act of dissolving, as when we say, that "solution is a chemical operation;" and the other, denoting the substance dissolved in its solvent, as "a solution of silver in nitrous acid." The French language is equally equivocal, as the word "dissolution" is used in both the above-mentioned senses. In treating on this subject, in which both meanings were very frequently required, sometimes in the same sentence, I could not but be sensible of confusion in the stile, and I have therefore confined the word solution to express the substance dissolved together with its solvent, and the word dissolution to denote the act of dissolving.—Orig.

we have only the choice of terms between words derived from the ancient theory, and those which have been lately proposed by the opposers of that theory. In this dilemma I have preferred the use of the former, not that I wish to show any predilection to either theory, but because that system, having long been generally adopted, is understood by all parties; and principally because, by using the words of the old theory, I am at liberty to define them, and to give significations expressive merely of facts, and of the actual state of bodies; whereas the language and theory of the antiphlogistic chemists being interwoven and adapted to each other, the former cannot be divested of its theoretical reference, and therefore seems inapplicable to the mere exposition of facts, but ought to be reserved solely for the explanation of the doctrines from which this language is derived. Thus, by the definition before mentioned of phlogistication, this word expresses not the presence or existence of an hypothetical principle of inflammability; but a certain well-known quality of acids and of other bodies, communicated to them by the addition of many actual inflammable substances. Thus nitrous acid acquires a phlogisticated quality by addition of a little spirit of wine, or by distillation with any inflammable substance.

3. No two substances are more frequently in the hands of chemists and artists than vitriolic acid and nitre, yet I have found, that a mere mixture of these, when much concentrated, possesses properties which neither the vitriolic acid nor the nitrous, of the same degree of concentration, have singly, and which could not easily be deduced, à priori, by reasoning from our present knowledge of the theory of chemistry.

4. Having found by some previous trials that a mixture composed of nitre dissolved in oil of vitriol was capable of dissolving silver easily and copiously, while it did not affect copper, iron, lead, regulus of cobalt, gold, and platina, I conceived, that it might be useful in some cases of the parting of silver from copper and the other metals above mentioned; and having also observed, that the dissolving powers of the mixture of vitriolic and nitrous acids varied greatly in different degrees of concentration and phlogistication, I thought that an investigation of these effects might be a subject fit for philosophical chemistry, and might tend to illustrate the theory of the dissolution of metals in acids. With these views I made the following experiments.

5. I put into a long-necked retort, the contents of which, including the neck, were 1400 grain measures, 100 grain measures of oil of vitriol of the usual density at which it is prepared in England, that is, whose specific gravity is to that of water as 1.844 to 1, and 100 grs. of pure and clean nitre, which was then dissolved in the acid by the heat of a water-bath. To this mixture 100 grs. of standard silver were added; the retort was set in a water bath, in which the water was made to boil, and a pneumatic apparatus was applied to catch any air

or gas which might be extricated.—The silver began to dissolve, and the solution became of a purple or violet colour. No air was thrown into the inverted jar, excepting a little of the common air of the retort, by means of the expansion which it suffered from the heat of the water-bath, and from some nitrous fumes which appeared in the retort, and which having afterwards condensed, occasioned the water to rise along the neck of the retort, and mix with the solution. The remaining silver was then separated and weighed, and it was found that 39 grains had been dissolved; but probably more would have been dissolved if the operation had not been interrupted by the water rushing into the retort.

6. In the same apparatus 200 grs. of standard silver were added to a mixture of 100 grs. of nitre, previously dissolved in 200 grain-measures of oil of vitriol; and in this solvent 92 grs. of the silver were dissolved, without any production of air or gas. The solution, which was of a violet colour, having been poured out of the retort while warm (for with so large a proportion of nitre, such mixtures, especially after having dissolved silver, are apt to congeal with small degrees of cold,) in order to separate the undissolved silver from it, and having been returned into the retort without this silver, I poured 200 grs. of water into the retort, on which a strong effervescence took place between the solution and the water, and 3100 grain-measures of nitrous gas were thrown into the inverted jar. On pouring 200 grs. more of water into the retort, 600 grain-measures of the same gas were expelled. Further additions of water yielded no more gas; neither did the silver, when afterwards added to this diluted solution, give any sensible effervescence, or suffer a greater loss of weight than 2 grains.

7. In the same apparatus 100 grs. of standard silver were exposed to a mixture of 30 grs. of nitre dissolved in 200 grain-measures of oil of vitriol; and in this operation, 80 grs. of silver were dissolved, while at the same time 4500 grain-measures of nitrous gas were thrown into the inverted jar. When the undissolved silver was removed, 200 grs. of water were added to the solution, which was of a violet colour, and on the mixture of the 2 fluids an effervescence happened; but only a few bubbles of nitrous gas were then expelled.

8. In the same apparatus 100 grs. of standard silver were exposed to a mixture of 200 grain-measures of oil of vitriol, 200 grs. of nitre, and 200 grs. of water; and in this operation 20 grs. of the silver were dissolved without any sensible emission of air or gas.

9. In these experiments, the copper contained in the standard silver gave a reddish colour to the saline mass which was formed in the solution, and seemed to be a calx of copper interspersed through the salt of silver. I perceived no other difference between the effects of pure and standard silver dissolved in this acid.

10. I then exposed tin to the same mixture of oil of vitriol and nitre, in the

same apparatus, and in the same circumstances, taking care always to add more metal than could be dissolved, that, by weighing the remainder, the quantity capable of being dissolved might be found, as I had done with the experiments on silver: and the results were as follow.

11. No tin was dissolved nor calcined by the mixtures in the proportion of 200 grain-measures of oil of vitriol to 200 grs. of nitre; nor by any other mixture in the proportion of 200 grain-measures of oil of vitriol to 150 grs. of nitre, and consequently no gas was produced in either instance.

12. With a mixture in the proportion of 200 grain-measures of oil of vitriol and 100 grs. of nitre, the tin began soon to be acted on, and to be diffused through the liquor; but no extrication of gas appeared till the digestion had been continued 2 hours in boiling water; and then it took place, and gave a frothy appearance to the mixture, which was of an opaque white colour, from the powder of tin diffused among it. In this experiment the quantity of tin thus calcined was 73 grs., and the quantity of nitrous gas extricated during this action on the tin was 8500 grain-measures. Then, on pouring 200 grs. of water into the retort, a fresh effervescence took place between the water and the white opaque mass, and 4600 grain-measures of nitrous gas were thrown into the inverted receiver.

13. With a mixture in the proportion of 100 grain-measures of oil of vitriol to 30 grs. of nitre, 30 grs. of tin were dissolved or calcined, and the nitrous gas, which began to be extricated much sooner than in the last-mentioned experiment with a larger proportion of nitre, amounted to 6300 grain-measures. Water, added to this solution of tin, did not produce any effervescence.

14. With a mixture in the proportion of 200 grain measures of oil of vitriol, 200 grs. of nitre, and 200 grs. of water, 133 grs. of tin were acted on with an effervescence, which took place violently, and produced 6500 grain-measures of nitrous gas.

15. The several mixtures above mentioned, in different proportions of nitre and oil of vitriol, did, by the help of the heat of the water-bath, calcine mercury into a white or greyish powder. Nickel was also partly calcined and partly dissolved by these mixtures. I did not perceive that any other metal was affected by them, excepting that the surfaces of some of them were tarnished.

16. These mixtures of oil of vitriol and nitre were apt to congeal by cold, those especially which had a large proportion of nitre. Thus, a mixture of 1000 grain-measures of oil of vitriol and 480 grs. of nitre, after having kept fluid several days, in a phial not so accurately stopped as to prevent altogether the escape of some white fumes, congealed at the temperature of 55° of Fahrenheit's thermometer; whereas some of the same liquid, having been mixed with equal parts of oil of vitriol, did not congeal with a less cold than 45°. The

congelation is promoted by exposure to air, by which white fumes rise, and moisture may be absorbed, or by any other mode of slight dilution with water.

17. Dilution of this compound acid, with more or less water, alters considerably its properties, with regard to its action on metals. Thus it has been observed, that in its concentrated state it does not act on iron; but by adding water, it acquires a power of acting on that metal, and with different effect according to the proportion of the water added. Thus, by adding to 2 measures of the compound acid 1 measure of water, the liquor is rendered capable of calcining iron, and forming with it a white powder, but without effervescence. With an equal measure of water effervescence was produced. With a larger proportion of water the iron gave also a brown colour to the liquor, such as phlogisticated nitrous acid acquires from iron, or communicates to a solution of martial vitriol in water.

18. Dilution with water renders this compound acid capable of dissolving copper and zinc, and probably those other metals which are subject to the action of the dilute vitriolic or nitrous acids.

§ 2. *An account of a new process for separating silver from copper.*—19. The properties of this liquor, in dissolving silver easily, without acting on copper, have rendered it capable of a very useful application in the arts. Among the manufactures at Birmingham, that of making vessels of silver plated on copper is a very considerable one. In cutting out the rolled plated metal into pieces of the required forms and sizes, there are many shreds, or scraps as they are called, unfit for any purpose but the recovery of the metals by separating them from each other. The easiest and most economical method of parting these 2 metals, so as not to lose either of them, is an object of some consequence to the manufacturers. For this purpose 2 modes were practised, 1, by melting the whole of the mixed metals with lead, and separating them by eliquation and testing; and the 2d, by dissolving both metals in oil of vitriol, with the help of heat, and by separating the vitriol of copper, by dissolving it in water, from the vitriol of silver, which is afterwards to be reduced and purified. In the first of these methods, there is a considerable waste of lead and copper; and in the 2d, the quantity of vitriolic acid employed is very great, as much more is dissipated in the form of volatile vitriolic, or sulphureous acid, than remains in the composition of the 2 vitriols.

Some years ago I communicated to an artist the method of effecting the separation of silver and copper by means of the above-mentioned compound of vitriolic acid and nitre; and, as I am informed, that it is now commonly practised by the manufacturers in Birmingham, I have no doubt but it is much more economical, and it is certainly much more easily executed, than any of the other methods: for nothing more is required than to put the pieces of plated metal into an

earthen glazed pan; to pour on them some of the acid liquor, which may be in the proportion of 8 or 10 lbs. of oil of vitriol to 1 lb. of nitre; to stir them about, that the surfaces may be frequently exposed to fresh liquor, and to assist the action by a gentle heat from 100° to 200° of Fahrenheit's scale. When the liquor is nearly saturated, the silver is to be precipitated from it by common salt, which forms a luna cornea, easily reducible by melting it in a crucible with a sufficient quantity of pot-ash; and lastly, by refining the melted silver, if necessary, with a little nitre thrown on it. In this manner the silver will be obtained sufficiently pure, and the copper will remain unchanged. Otherwise, the silver may be precipitated in its metallic state, by adding to the solution of silve a few of the pieces of copper, and a sufficient quantity of water to enable the liquor to act on the copper. The property which this acid mixture possesses of dissolving silver with great facility, and in considerable quantity, will probably render it a useful menstruum in the separation of silver from other metals; and as the alchemists have distinguished the peculiar solvent of gold under the title of aqua regis, a name sufficiently distinctive, though founded on a fanciful allusion; so, if they had been acquainted with the properties of this compound, they would probably have bestowed on it the appellation of aqua reginæ.

§ 3. *The change of properties communicated to the mixture of vitriolic and nitrous acids by phlogistication.*—20. The above-described compound acid may be phlogisticated in different methods, of which I shall mention 3. 1st, By digesting the compound acid with sulphur by means of the heat of a water-bath, the liquor dissolves the sulphur with effervescence, loses its property of yielding white fumes; and if the quantity of sulphur be sufficient, and if the heat applied be long enough continued, it exhibits red nitrous vapours, and assumes a violet colour.

2dly, If, instead of dissolving nitre in concentrated vitriolic acid, this acid be impregnated with nitrous gas, or with nitrous vapour, by making this gas, or vapour pass into the acid, this compound will be phlogisticated, as it contains not the entire nitrous acid, but only its phlogisticated part, or element, the nitrous gas, without the proportion of pure air is necessary to constitute an acid. This impregnation of oil of vitriol with nitrous gas, or nitrous vapour, was first described, and some of the properties of the impregnated liquor noticed, by Dr. Priestley. See Exp. and Obs. on Air, vol. 3, p. 129 and 217. 3dly, By substituting nitrous ammoniac instead of nitre in the mixture with oil of vitriol.

21. The compound prepared by any of these methods, but especially by the 1st and 2d, differs considerably in its properties with regard to its action on metals from the acid described in the first section. It has been observed, that the latter compound has little action on any metals but silver, tin, mercury, and nickel. On the other hand, the phlogisticated compound not only acts on these,

but also on several others. It forms with iron a beautiful rose-coloured solution, without application of any artificial heat; and in time a rose-coloured saline precipitate is deposited, which is soluble in water with considerable effervescence. It dissolves copper, and acquires from this metal, and also from regulus of cobalt, zinc, and lead, pretty deep violet tinges. Bismuth and regulus of antimony were also attacked by this phlogisticated acid. To ascertain more exactly the effects of this phlogisticated acid on some metals, I made the following experiments, with a liquor prepared by making nitrous gass pass through oil of vitriol during a considerable time.

22. To 200 grain-measures of the oil of vitriol impregnated with nitrous gas, put into a retort with a long neck, the capacity of which, including the neck, was 1150 grain-measures, I added 144 grs. of standard silver, and immersed the mouth of the retort in water, under an inverted jar filled with water, to catch the gas which might be extricated. The acid began to dissolve the silver with effervescence without application of heat; the solution became of a violet colour, and the quantity of nitrous gas received in the inverted jar was 14700 grain-measures. On weighing the silver remaining, the quantity which had been dissolved was found to be 79 grs. When water was added to the solution, an effervescence appeared, but only a very small quantity of gas was extricated. By means of the water, a white saline powder of silver, soluble in a larger quantity of water, was precipitated from the solution. The solution of silver, when saturated and undiluted, congeals readily in cool temperatures, and, when diluted to a certain degree with water, gives foliated crystals.

23. In the same apparatus, and in the same manner, 100 grain-measures of this impregnated oil of vitriol were applied to iron. An effervescence appeared without application of heat, the surface of the iron acquired a beautiful rose colour or redness mixed with purple: and this colour gradually pervaded the whole liquor, but disappeared on keeping the retort some time in hot water. Notwithstanding a considerable apparent effervescence, the quantity of air expelled in the inverted jar was only 400 grain-measures, of which $\frac{1}{4}$ was nitrous, and the rest phlogisticated. The solution was then poured out of the retort, and the iron was found to have lost only 2 grs. in weight. The solution was returned into the retort, without the iron, and 200 grs. of water were added to it; on which a white powder was immediately precipitated, which re-dissolved with great effervescence. When 2000 grain-measures of nitrous gas had been expelled in the inverted jar, without application of heat, the retort was placed in the water-bath, the heat of which rendered the effervescence so strong, that the liquor boiled over the neck of the retort, so that the quantity of gas extricated could not be ascertained.

24. In the same manner 11 grs. of copper were dissolved in 100 grain-

measures of impregnated oil of vitriol. The solution was of a deep violet colour, and at last was turbid. The quantity of nitrous gas expelled into the inverted jar during the operation was 4700 grain-measures. When the copper was removed, and 200 grs. of water were added to the solution, an effervescence took place, 1700 grain-measures of nitrous gas were expelled, and the solution then acquired a blue colour.

25. In the same apparatus and manner, 100 grain-measures of the impregnated oil of vitriol were applied to tin, which was thence diminished in weight 16 grs., while the liquor acquired a violet colour, became turbid by the suspension of the calx of tin, and a quantity of nitrous gas was thrown into the inverted receiver equal to 4100 grain-measures, without application of heat, and another quantity equal to 4900 grain-measures, after the retort was put into the water-bath.

26. Mercury added to the impregnated oil of vitriol formed a thick white turbid liquor, which was rendered clear by addition of unimpregnated oil of vitriol. In a little time this mixture continuing to act on the remaining mercury acquired a purple colour. The mercury acted on sunk to the bottom of the glass in the form of a white powder, and the purple liquor, when mixed with a solution of common salt in water, gave no appearance of its containing any mercury in a dissolved state.

27. The nitrous gas with which the oil of vitriol is impregnated shows no disposition to quit the acid by exposure to air; but, on adding water to the impregnated acid, the gas is expelled suddenly with great effervescence, and with red fumes, in consequence of its mixture with the atmospherical air. On adding 240 grs. of water to 60 grain-measures of impregnated oil of vitriol, 2300 grs. of nitrous gas were thrown into the receiver; but as the action of the 2 liquors is instantaneous, the quantity of gas expelled from the retort before its neck could be immersed in water, and placed under the receiver, must have been considerable. The whole of the gas however was not extricated by means of the water, for the remaining liquor dissolved 5 grs. of copper, while 800 measures of nitrous gas were thrown into the retort.

28. The following facts principally are established by the preceding experiments. 1. That a mixture of the vitriolic and nitrous acids in a concentrated state has a peculiar faculty of dissolving silver copiously. 2. That it acts on, and principally calcines, tin, mercury, and nickel; the latter of which however it dissolves in small quantity: and that it has little or no action on other metals. 3. That the quantity of gas produced while the metal is dissolving is greater, relatively to the quantity of metal dissolved, when the proportion of nitre to the vitriolic acid is small, than when it is large; and that when the metals are dissolved by mixtures containing much nitre, and with a small production of gas,

the solution itself, or the metallic salt formed in it, yields abundance of gas when mixed with water. 4. That dilution with water renders the concentrated mixture less capable of dissolving silver, but more capable of acting on other metals. 5. That this mixture of highly concentrated vitriolic and nitrous acids acquires a purple or violet colour when phlogisticated, either by addition of inflammable substances, as sulphur, or by its action on metals, or by very strong impregnation of oil of vitriol with nitrous gas*. 6. That this phlogistication was found to communicate to the mixture the power of dissolving, though in small quantities copper, iron, zinc, and regulus of cobalt. 7. That water expels from a highly phlogisticated mixture of concentrated vitriolic and nitrous acids, or of oil of vitriol impregnated with nitrous gas, a great part of its contained gas; and that therefore this gas is not capable of being retained in such quantity by dilute as by concentrated acids. Water unites with the mixture of oil of vitriol and nitre, without any considerable effervescence.

29. To these observations I shall subjoin one other fact, namely, that when, to the mixture of oil of vitriol with nitre, a saturated solution of common salt in water is added, a powerful aqua regis is produced, capable of dissolving gold and platina; and this aqua regis, though composed of liquors perfectly colourless and free from all metallic matter, acquires at once a bright and deep yellow colour. The addition of dry common salt to the concentrated mixtures of vitriolic and nitrous acids produces an effervescence, but not the yellow colour; for the production of which therefore a certain proportion of water seems to be necessary.

PART 2. *On the precipitation of silver from nitrous acid by iron.*

§ 1. Bergman relates, that on adding iron to a solution of silver in the nitrous acid, no precipitation ensued; though the affinity of iron to acids in general is known to be much stronger than that of silver; and though, even with regard to the nitrous acid, other experiments evince the superior affinity of iron: for as iron precipitates copper from this acid, and as copper precipitates silver, we must infer the greater affinity of iron than of silver. In the course of his experiments however, some instances of precipitation occurred, which he attributed to the peculiar quality of the irons which he then employed†. I was desirous of dis-

* Dr. Priestley has noticed this colour communicated to oil of vitriol by impregnation with nitrous gas or vapour, and also the effervescence produced by adding water to this impregnated liquor. See Exp. and Obs. vol. 3, p. 129 and 217.—Orig.

† Bergman tried many different kinds of iron, and he thought he found 2 that were capable of precipitating silver. But as he did not discover the circumstances according to which this precipitation sometimes does, and at other times does not happen, he may have been mistaken with regard to the peculiar quality of these 2 kinds of iron. At least the several kinds which I have tried always precipitated silver in certain circumstances, and always failed to precipitate in certain other circumstances. I do not know any other author who has mentioned this subject, excepting Mr. Kirwan; who, in the conclusion of his valuable papers on the Attractive Powers of Mineral Acids, says, "I have always found silver to be easily precipitated from its solution in the nitrous acid by iron. The sum of the

covering the circumstances, and of investigating the cause, if I should be able, of this irregularity and exception to the generally received laws of affinity.

2. I digested a piece of fine silver in pure and pale nitrous acid, and while the dissolution was going on, and before the saturation was completed, I poured a portion of the solution on pieces of clean and newly-scraped iron wire into a wine glass, and observed a sudden and copious precipitation of silver. The precipitate was at first black, then it assumed the appearance of silver, and was 5 or 6 times larger in diameter than the piece of iron wire which it enveloped. The action of the acid on the iron continued some little time, and then it ceased; the silver re-dissolved, the liquor became clear, and the iron remained bright and undisturbed in the solution at the bottom of the wine glass, where it continued during several weeks, without suffering any change, or affecting any precipitation of the silver.

3. When the solution of silver was completely saturated, it was no longer affected by iron, according to Bergman's observation.

4. Having found that the solution acted on the iron, and was thus precipitated, before it had been saturated, and not afterwards, I was desirous of knowing, whether the saturation was the circumstance which prevented the action and precipitation. For this purpose I added to a portion of the saturated solution some of the same nitrous acid, of which a part had been employed to dissolve the silver; and into this mixture, abounding with a superfluous acid, I threw a piece of iron, but no precipitation occurred. It was thence evident, that the saturation of the acid was not the only circumstance which prevented the precipitation.

5. To another portion of the saturated solution of silver I added some red smoking nitrous acid; and I found, on trial, that iron precipitated the silver from this mixture, and that the same appearances were exhibited as had been observed with the solution before its saturation.

quiescent affinities being 625, and that of the divellent 746. Yet Mr. Bergman observed, that a very saturated solution of silver was very difficultly precipitated, and only by some sorts of iron, even though the solution was diluted, and an access of acid added to it. The reason of this curious phenomenon appears to me deducible from a circumstance first observed by Scheele, in dissolving mercury, namely, that the nitrous acid when saturated with it will take up more of it in its metallic form. The same thing happens in dissolving silver in the nitrous acid in a strong heat; for, as I before remarked, the last portions of silver thrown in afford no air, and consequently are not dephlogisticated. Now this compound of calx of silver, and silver in its metallic form, may well be unprecipitable by iron, the silver in its metallic form preventing the calx from coming into contact with the iron, and extracting phlogiston from it." In this paper I shall not enter into the explanation of these appearances; but I thought it necessary to premise what so eminent a chemist as Mr. Kirwan has suggested on the subject, that the reader may see at once the present state of the question. I shall only remark, that the above explanation, not being founded on any peculiarity in the nature of iron, seems to suppose that the silver is also incapable of being precipitated from such solutions as iron cannot act on by any other metal. But this is not the case: copper and zinc readily precipitate silver from these solutions.—Orig.

6. The same effects were produced when vitriolic acid was added to the saturated solution of silver, and iron afterwards applied.

7. To some of the same nitrous acid, of which a part had been employed to dissolve the silver, I added a piece of iron; and while the iron was dissolving I poured into the liquor some of the saturated solution of silver; on which a precipitation of silver took place instantly; though, when the same acid had been previously mixed with the solution of silver, and the iron was then added to the mixture, no precipitation had ensued.

8. The quantity of vitriolic acid, or of the red fuming nitrous acid, necessary to communicate to the saturated solution of silver the property of being acted on by iron, varies according to the concentration, and to the degree of phlogistication of the acids added; so that a less quantity than is sufficient does not produce any apparent effect. Yet, when the solution of silver is by addition of these acids brought nearly to a precipitable state, the addition of spirit of wine will, in a little time, render it capable of acting on iron.

9. It appears then, that a solution of silver is not precipitated by iron in cold, unless it have a superabundance of phlogisticated acid.*

10. Heat affects the action of a solution of silver on iron: for if iron be digested with heat, in a perfectly saturated solution of silver, such as a solution of crystals of nitre of silver in water, the silver will be deposited in its bright metallic state on different parts of the iron, and the iron which has been acted on by the solution appears in form of a yellow ochre.

11. Bergman relates, that he has sometimes observed beautiful crystallizations or vegetations of metallic silver formed on pieces of iron immersed long in a solution of silver. I have found that no time is able to effect this deposition, unless the solution be in a state nearly sufficiently phlogisticated to admit of a precipitation by iron, but not completely phlogisticated enough to effect that purpose immediately.

12. Dilution with a great deal of water seemed to dispose the solutions of silver to be precipitated by iron more easily. A solution of silver, which did not act on iron, on being very much diluted, and having a piece of iron immersed

* It was said, at § 4, that the addition of dephlogisticated nitrous acid to a saturated solution of silver did not render this solution precipitable by iron. Yet, as this acid dissolves iron, such a quantity may be added, as to overcome the counteracting quality of the solution of silver, so that the acid shall be able to act on the iron; and while this metal is dissolving, it phlogisticates the mixture, which then becomes capable of being precipitated, and is in fact reduced to the same circumstances as are described at § 7. The limits of the quantities which produce changes cannot be ascertained, because they depend on the degrees of concentration and phlogistication of the substances employed; and therefore, whenever a change is said to be produced by a certain substance, it means that it may be produced by some proportion, but does not imply by every proportion, of that substance. Without attending to these considerations, persons trying to repeat the experiments mentioned in this paper will be liable to be deceived.—Orig.

in it, during several hours, gave a precipitate of silver in the form of a black powder.

§ 2. *On the alterations which iron or its surface undergoes by the action of a solution of silver in nitrous acid, or of a pure concentrated nitrous acid.*—13. It has been said, that when iron is exposed to the action of a phlogisticated solution of silver, it instantly precipitates the silver, is itself acted on or dissolved by the acid solution during a certain time, longer or shorter, according to the degree of phlogistication, quantity of superabundant acid, and other circumstances, and that at length the solution of the iron ceases; the silver precipitate is re-dissolved, if there is superfluous acid; the liquor becomes clear again, but only rendered a little browner by its having dissolved some iron; while the piece of iron remains bright and undisturbed at the bottom of the liquor, where it is no longer able to affect the solution of silver.

14. I poured a part of the phlogisticated solution of silver which had passed through these changes, and which had ceased to act on the piece of iron, into another glass, and dropped another piece of fresh iron wire into the liquor; on which I observed a precipitation of silver, a solution of part of the iron, a re-dissolution of the precipitated silver, and a cessation of all these phenomena, with the iron remaining bright and quiet at the bottom of the liquor, as before. It appeared then, that the liquor had not lost its power of acting on fresh iron, though it ceased to act on that piece which had been exposed to it.

15. To one of the pieces of iron which had been employed in the precipitation of a solution of silver, and from which the solution, no longer capable of acting on it, had been poured off, I added some phlogisticated solution of silver which had never been exposed to the action of iron, but no precipitation happened. It appeared then, that the iron itself, by having been once employed to precipitate a solution of silver, was rendered incapable of any further action on any solution of silver. And it is to be observed, that this alteration was produced without the least diminution of its metallic splendour, or change of colour. The alteration however was only superficial, as may be supposed; for by scraping off its altered coat, it was again rendered capable of acting on a solution of silver. To avoid circumlocution, I shall call iron thus affected, altered iron; and iron which is clean, and has not been altered, fresh iron.

16. To a phlogisticated solution of silver, in which a piece of bright altered iron lay, without action, I added a piece of fresh iron, which was instantly enveloped with a mass of precipitated silver, and acted on as usual; but, what is very remarkable, in about a quarter of a minute, or less, the altered iron suddenly was covered with another coat of precipitated silver, and was now acted on by the acid solution like the fresh piece. In a little time the silver precipitate was re-dissolved, as usual, and the two pieces of iron were reduced to an altered state.

When a fresh piece of iron was then held in the liquor, so as not to touch the two pieces of altered iron, they were also soon acted on by the acid solution, and suddenly covered with silver precipitate as before; and these phenomena may be repeated with the same solution of silver, till the superfluous acid of the solution becomes saturated by the iron, and then the re-dissolution of the precipitated silver must cease.

17. I poured some dephlogisticated nitrous acid on a piece of altered iron, without any action ensuing, though this acid readily acted on fresh iron; and when, to the dephlogisticated nitrous acid, with a piece of altered iron lying immersed in it, I added a piece of fresh iron, this immediately began to dissolve, and soon afterwards the altered iron was acted on also by the acid.

18. On a piece of altered iron I poured a solution of copper in nitrous acid; but the copper was not precipitated by the iron; neither did this iron precipitate copper from a solution of blue vitriol.

19. Altered iron was acted on by a dilute phlogisticated nitrous acid; but not by a red concentrated acid, which is known to be highly phlogisticated.

20. I put some pieces of clean fresh iron wire into a concentrated and red fuming nitrous acid. No apparent action ensued; but the iron was found to be altered in the same manner as it is by a solution of silver; that is, it was rendered incapable of being attacked either by a phlogisticated solution of silver, or by dephlogisticated nitrous acid.

21. Iron was also altered by being immersed some little time in a saturated solution of silver, which did not show any visible action on it.

22. The alteration thus produced on the iron is very superficial. The least rubbing exposes some of the fresh iron beneath the surface, and thus subjects it to the action of the acid. It is therefore with difficulty that these pieces of altered iron can be dried, without losing their peculiar property. For this reason, I generally transferred them out of the solution of silver, or concentrated nitrous acid, into any other liquor, the effects of which I wanted to examine. Or they may be transferred first into a glass of water, and thence into the liquor to be examined. But it is to be observed, that if they are allowed to remain long in the water, they lose their peculiar property or alteration. They may be preserved in their altered state by being kept in spirit of sal ammoniac.

23. To a saturated solution of copper in nitrous acid, which was capable of being readily precipitated by fresh iron, I added some saturated solution of silver. From this mixture a piece of fresh iron neither precipitated silver nor copper: nor did the addition of some dephlogisticated nitrous acid effect this precipitation.

24. A solution of copper, formed by precipitating silver from nitrous acid by means of copper, was very reluctantly and slowly precipitated by a piece of fresh iron; and the iron thus acted on by the acid was changed to an ochre.

25. A saturated solution of silver having been partly precipitated by copper, acquired the property of acting on fresh iron, and of being precipitated by it.

26. Fresh iron immersed some time in solutions of nitre of lead, or of nitre of mercury in water, did not occasion any precipitation of the dissolved metals; but acquired an altered quality. These metals then in this respect resemble silver.

27. It is well known, that a solution of martial vitriol, added to a solution of gold in aqua regis, precipitates the gold in its metallic state. I do not recollect, that the precipitation of a solution of silver by the same martial vitriol has been observed. However, on pouring a solution of martial vitriol into a solution of silver in the nitrous acid, a precipitate will be thrown down, which acquires in a few minutes more and more of a metallic appearance, and is indeed perfect silver. When the 2 solutions are pretty concentrated, a bright argentine film swims on the surface of the mixture, or silvers the sides of the glass in which the experiment is made. When a phlogisticated solution of silver is used, the mixture is blackened, as happens generally to a solution of martial vitriol, when a phlogisticated nitrous acid is added to it.

I added about equal parts of water to a mixture of a phlogisticated solution of silver and a solution of martial vitriol, in which all the silver had been precipitated, and digested the diluted mixture with heat, by which means most of the precipitated silver was re-dissolved. Bergman has observed a similar re-dissolution of gold precipitated by martial vitriol on boiling the mixture; but he attributes the re-dissolution to the concentration of the aqua regis by the evaporation. As this explanation did not accord with my notions, I diluted the mixture with water, and found that the same re-dissolution occurred both with the solution of silver and with that of gold. But with neither of the metals did I find that the re-dissolution ever took place, unless there had been a superabundant acid in the solutions of gold and silver employed.

28. Mercury is also precipitated in its metallic state from its solution in nitrous acid by a solution of martial vitriol. When the liquor is poured off from the precipitate, this may be changed into running mercury by being dried near the fire.

29. I found also, that silver may be precipitated in its metallic state, from its solution in vitriolic acid, by addition of a solution of martial vitriol. A vitriol of mercury may also be decompounded by a solution of martial vitriol, and the mercurial precipitate, which is a black powder, forms globules, when dried and warmed.

30. Luna cornea is not decompounded by martial vitriol; consequently there is no operation of a double affinity. Yet this luna cornea may be decompounded by the elements of martial vitriol, while they are in the act of dissolution; that is, the silver may be precipitated in its metallic state, by digesting luna cornea with a dilute vitriolic acid, to which some pieces of iron are added. And it is to be observed, that this reduction of the silver and precipitation take place, while

the acid is yet unsaturated. Marine acid and iron applied to luna cornea effect the same reduction of the silver to a metallic state, even when there is more acid than is sufficient for both metals.

The explanation of these phenomena will be attempted in the subsequent papers which I propose to present on this subject to the Society.

XXI. Determination of the Longitudes and Latitudes of some Remarkable Places near the Severn. By Edward Pigott, Esq. p. 385.

Difference of longitudes in time between Greenwich and Frampton-house, deduced from observed meridian transits of the moon's limbs. There were 13 of these observations, from all of which the medium is $14^m\ 32^s$ for the difference of long. between those two places. This method of determining terrestrial longitudes Mr. P. had detailed in the Philos. Trans. vol. 76, and still thinks it cannot be too strongly recommended. The latitude of the same place, taken with an 18-inch quadrant made by Bird, by a medium of 7 observations, was $51^\circ\ 24'\ 58''$. The same as given by his father in the Philos. Trans. vol. 71, is $51^\circ\ 25'\ 1''$; the mean of both he takes at $51^\circ\ 25'\ 0''$, for the latitude of the observatory at Frampton-house.

Having thus settled the position of the Observatory, Mr. P. next proceeds to give the particulars of the trigonometrical operations. He measured the same base 3 times by different methods, the results were 2046, 2042, 2042 feet. As the view from its extremities was very confined, another base of 1861 yards was deduced from it, situated on the high lands that edge the Severn. From the extremities of this 2d base, all the angles were taken with a tolerably good theodolite, on which 2' might be easily read off. The results here given are the distances from the various places to the western extremity of their base, their perpendicular distances to its meridian, and its distance from these perpendiculars.

Distances in yards,					
Direct.	To the meridian.		To the perpendicular.		
3307	1254	E	3039	N	Frampton-house.
45654	42239	E	17324	S	Brin Hill, the centre.
36928	21853	E	29768	S	Quantock Hill, the east part.
40446	15586	E	37322	S	Land Mark, a tower.
35543	11542	E	33617	S	Watchet Hill, the centre.
21911	1465	E	21862	S	Minehead.
21336	6664	W	20268	S	Porlock, or Huston Point.
30238	20152	W	19450	S	Leemouth.
46254	40398	W	22547	S	Hangman Hill.
2921	2842	W	673	N	St. Donat's Castle.
1564	491	E	1483	N	Llantwit Church.
10140	448	W	10130	N	Llangwynewar Hill, east part.
25126	2299	E	25020	N	A remarkable hill.
3135	2063	E	2361	N	Llanmace Church.
8864	5906	E	6609	N	St. Hilary's Church.

The direct distances are the most accurate, the others being affected according to the exactness of the meridian of the west extremity of the base; the direction of which was found by the variation needle, its declination having been determined at Frampton-house, and therefore sufficiently correct; for an error in that angle, even of half a degree, would make a difference of a very few seconds in any of the places observed.

The following are the longitudes and latitudes of the same places, deduced by Gen. Roy's most accurate and useful tables, showing the value of each degree, &c.

Longitudes west of Greenwich,					Latitudes North.			
in time.		in deg. &c.						
m.	s.	°	′	″	°	′	″	
12	24 −	3	5	58	51	14	56½	Brin Hill, the centre.
13	28 −	3	21	57	51	8	48½	Quantock Hill, east part.
13	47½	3	26	52	51	5	5	Land-mark, a tower.
14	0 +	3	30	1	51	6	55	Watchet Hill, the centre.
14	17½	3	34	23	51	26	44½	St. Hilary's Church.
14	29 −	3	37	12	51	35	49	A remarkable hill.
14	29½	3	37	24	51	24	39	Llanmace Church.
14	31½	3	37	52	51	12	42½	Minehead.
14	32 +	3	38	2	51	25	0 −	Frampton-house.
14	34½	3	38	38	51	24	13	Llantwit Church.
14	36 +	3	39	1	51	23	29	Station, west extremity of the base.
14	37½	3	39	22	51	28	28½	Llangwynewar Hill, east part.
14	45	3	41	15	51	23	49	St. Donat's Castle.
14	57 −	3	44	14	51	13	29½	Porlock or Huston Point.
15	48½	3	57	7	51	13	54	Leemouth.
16	42½	4	10	35	51	12	22	Hangman Hill.

XXII. Experiments and Observations on the Matter of Cancer, and on the Aërial Fluids extricated from Animal Substances by Distillation and Putrefaction; together with some Remarks on Sulphureous Hepatic Air. By Adair Crawford, M. D., F. R. S.* p. 391.

There are several varieties in the colour and consistence of the matter discharged by cancerous ulcers. It is in some cases of a pale ash colour; in others, it has a reddish cast; and in many instances it has more or less of a brown tinge, sometimes approaching nearly to black. Its consistence is for the most part thin; but in the cancerous, as well as in the other malignant ulcers, we frequently meet with a white sordes, which closely adheres to the surface of the sore, and which appears to be scarcely miscible with water. In the same patient the appearance of the discharge is frequently varied by internal remedies, or by external applications; but if we except the temporary variations produced by accidental circumstances, the cancerous ulcer is, in its advanced stage, very generally ac-

* Author of a very ingenious philosophical treatise on Animal Heat.

companied with a peculiar odour more highly fetid and offensive than that which is emitted by other malignant ulcers.

Apprehending that some light might be thrown on the nature of cancerous diseases, by inquiring into the properties of this substance, Dr. C. procured a portion of it from a patient who had for several years been afflicted with a cancer in the breast. Having diffused it through pure water, he divided it into 3 parts, which were put into small glass vessels. To one of these he added a solution of vegetable fixed alkali; to the 2d, a little concentrated vitriolic acid; and to the 3d, syrup of violets. By the vegetable fixed alkali no sensible change was produced: on the addition of the vitriolic acid, the liquor in the 2d glass acquired a deep brown colour, a brisk effervescence took place, and at the same time the peculiar odour of the cancerous matter was greatly increased, and diffused itself to a considerable distance through the surrounding air. The syrup of violets communicated to the liquor in the 3d glass a faint green colour. The cancerous matter used in these experiments had a brownish cast. It had been imbibed by cotton, and kept for some days before the trials were made.

Mr. Geber has shown, that animal substances on their first putrefaction do not effervesce with acids; that after the process has continued for some time, a manifest effervescence takes place; and that this effect again disappears before the putrefaction has ceased. Suspecting that the effervescence in the preceding experiment might have arisen from a change which the matter underwent, in consequence of its having been kept some days before the trial was made, Dr. C. repeated the experiment with a portion of reddish matter recently obtained from a cancerous penis. On the addition of the acid, the liquor, as before, acquired a brown colour, its fetor was much increased, and a manifest effervescence took place, though it was not so considerable as in the former instance. A portion of the same matter diffused through distilled water communicated a blue tinge to tincture of litmus, and a greenish cast to syrup of violets. It is proper to observe, that when syrup of violets was mixed with portions of cancerous matter from a variety of different subjects, the change produced was in some cases scarcely perceptible; but in every instance the presence of an alkali was detected by dipping into the matter a slip of paper that had been previously tinged blue by tincture of litmus, and afterwards slightly reddened by acetous acid. The red colour was invariably in the course of a few minutes abolished, and the blue restored.

The cancerous matter, as has been already remarked, acquired, on the addition of the vitriolic acid, a brown hue. It is well known, that this acid, when it is highly concentrated, communicates a brown or black colour to all animal and vegetable substances. Being desirous of learning whether the change which took place on the addition of the acid to the cancerous matter in this experiment, was different from that which would be produced by the same acid in other animal

substances, and particularly in recent healthy pus; Dr. C. took equal quantities of the latter, and of ash-coloured cancerous matter, and having diffused each of them through thrice its weight of distilled water, he added to them equal quantities of concentrated vitriolic acid; the weight of the acid being nearly the same with that of the matter used in the experiment. The mixture containing the pus acquired from the acid a faint brown colour; but that which contained the cancerous matter, was suddenly changed to a deep brown, approaching to black. When these mixtures were diluted with about twice their weight of distilled water, the brown tinge of the former entirely disappeared; but the latter still retained its brown colour, though it was somewhat fainter than it had been on the first addition of the acid.

The aërial fluid which was disengaged in the foregoing trials from the matter of cancer, by the vitriolic acid, appeared from its odour to have a nearer resemblance to hepatic than to any other species of air. As it seemed, from its sensible qualities, to be a very active, and probably a deleterious principle, he endeavoured more particularly to inquire into its nature, and to compare it with common hepatic air. But before relating the trials which were made with that view, it may not be improper briefly to mention the characters by which common hepatic air is distinguished. It has a smell resembling that of rotten eggs; it is inflammable, and during its combustion in the open air, sulphur is deposited; it communicates a black colour to silver and copper, and a brownish tinge to lead and iron; it is soluble in water, and when a solution of nitrated silver is dropped into water impregnated with it, the mixture becomes turbid, and a dark-coloured precipitate falls to the bottom; by the addition of the nitrated silver, the odour of the hepatic air is rendered much fainter; and it is entirely destroyed by concentrated nitrous, or by dephlogisticated marine acid.

To determine whether the aërial fluid contained in the cancerous matter possessed these properties, a portion of this substance was diffused through distilled water. The mixture being filtered, a small quantity of nitrated silver was dropped into it. In a little time, an ash-coloured cloud was produced, which soon afterwards acquired a brownish purple hue, and at the end of 2 hours the colour of the mixture was changed to a deep brown. The fetid smell was now rendered much fainter than that of a similar mixture of cancerous matter, and of distilled water, to which nitrated silver had not been added. When a little concentrated nitrous acid was dropped into the mixture which had been thus altered by the addition of nitrated silver, a slight effervescence took place, the brown hue was instantly changed to an orange colour, and the fetid smell was abolished. The fetor was likewise entirely destroyed, when dephlogisticated marine acid was added either to cancerous matter in its separate state, or to a portion of that substance which had been previously mixed with nitrated silver.

By the foregoing properties the cancerous virus is distinguished from common pus: for when dilute vitriolic acid is added to common pus, no effervescence is produced; and when a solution of nitrated silver is dropped into this substance previously diffused through distilled water, the mixture does not acquire a brown colour; nor does any sensible precipitation take place for several hours. It appeared however, that when the last experiment was repeated with matter obtained from a venereal bubo, the mixture on the addition of the nitrated silver became slightly turbid, and, at the end of 2 hours, it acquired a brownish cast. The same effects were perceived when the trial was made with matter obtained from a carious bone. But in these instances the precipitation was much less considerable than that which was produced by the cancerous matter.

Dr. C. next endeavoured to procure, in its separate state, a portion of the air which is extricated from the matter of cancer by the vitriolic acid. With this intention a quantity of reddish cancerous matter was mixed in a small proof, with about thrice its weight of distilled water. To this mixture a little vitriolic acid was added; on which an effervescence took place, and the air that was disengaged was received in a phial over mercury. When one half of the mercury was expelled from the phial, the latter was inverted over distilled water, and the portion of the mercury that remained in it being suffered to descend, and the water to rise into its place, the phial was closely corked. The air and water were then briskly agitated together, and the phial being a 2d time inverted over distilled water, the cork was removed; when it appeared by the height to which the water rose, that a part of the air had been absorbed. The water contained in the phial was now found to be strongly impregnated with the odour of the cancerous matter, and a little nitrated silver being dropped into it, a purplish cloud, inclining to red, was produced. It is proper to observe, that the change of colour on the addition of the nitrated silver, in this experiment, was at first scarcely perceptible; but in the course of a few minutes it became very distinct. As it might perhaps be doubtful, whether this alteration would not be produced in the nitrated silver by exposure to the air alone, the colour of the mixture was compared with that of a similar mixture of nitrated silver and of pure distilled water, which had remained exposed to the open air for an equal length of time. Though a slight change of colour was produced in the latter instance, yet it was much less considerable than that which took place in the former. In the above recited experiment, the air came over mixed with the common air that was contained in the proof. The quantity of aërial fluid that can be thus extricated by the addition of the acid without the assistance of heat, is not very considerable. If heat be applied, a larger portion of fetid air, having the odour of cancerous matter, may be disengaged; but in that case it will be found to be mixed with vitriolic acid air.

With a view to obtain the former of these fluids in as pure a state as possible,

the experiment was repeated in the following manner. A portion of the cancerous virus, diffused through distilled water, was introduced into a small proof; a little vitriolic acid was added; the vessel was filled with distilled water, and a crooked tube, also filled with that fluid, was fixed to its neck. The extremity of the tube being then introduced into the mouth of an inverted bottle containing water, and the flame of a candle being applied to the bottom of the proof, a quantity of air was expelled, which was received in the bottle. This air, when it was first disengaged, rose in the form of white bubbles; it had a very fetid smell, similar to that of the cancerous matter; and the water which was impregnated with it occasioned a dark-brown precipitate in a solution of nitrated silver. The crooked tube being separated from the proof, a very offensive white vapour, resembling in its odour the air extricated during the experiment, arose from the mixture, and continued to ascend for nearly half an hour. When to a portion of this smoking liquor, previously filtered, a little concentrated nitrous acid was added, the fetid smell was entirely destroyed, a slight effervescence took place, and a flaky substance that floated through the mixture was disengaged.

The foregoing experiments prove, in general, that the fetid odour of the matter of cancer is increased by the vitriolic, but entirely destroyed by the concentrated nitrous and dephlogisticated marine acids; that the aërial fluid, which is disengaged by the vitriolic acid, is soluble in water, and that the solution deposits a reddish brown precipitate on the addition of nitrated silver. Whence it follows, that the cancerous matter contains a principle which has many of the properties of hepatic air, and which may perhaps not improperly be termed animal hepatic air. It has also been shown, that the matter of cancer is impregnated with an alkali which is in such a state as to change the colour of vegetable tinctures. Dr. C. had very little doubt that this was the volatile alkali: for it is well known, that putrid animal substances frequently abound with that salt; but have never, he believes, been found to contain a fixed alkali in a disengaged state. With a view however, more decisively to determine this point, he tried the following experiment. A quantity of cancerous matter, diffused through distilled water, was introduced into a glass retort to which a receiver was adapted. The mixture was slowly distilled by means of a sand heat; and a small quantity of the liquor which came over into the receiver being poured into an infusion of Brazil wood, instantly imparted to it a deep red colour. Hence it clearly appears, that the alkali contained in the cancerous matter was the volatile, because it was separated by distillation with a heat which did not exceed that of boiling water.

It seemed extremely probable, that the above-mentioned alkali was united to the aërial fluid with which the matter of cancer is impregnated. Of the truth of this fact he was persuaded by observing, that the smell of the cancerous matter was greatly increased by the addition of the vitriolic acid: for he could scarcely

avoid concluding, that this phenomenon arose from an union between the acid and alkali, in consequence of which the odoriferous principle was extricated by a superior attraction. This conclusion will be confirmed by experiments to be recited in the sequel, which prove, that the volatile alkali is capable of entering into a chemical combination with the aërial fluid contained in the matter of cancer.

Of the air extricated from cancerous matter, and from other animal substances, by distillation.—A portion of matter from a cancerous breast was diffused through distilled water, and introduced into a small coated glass retort, which was gradually exposed to heat in a sand bath till the bottom of the retort became red-hot. The neck of the latter was introduced below an inverted jar filled with water, and a quantity of air was received in the jar, which was found to consist of the common air contained in the retort. Two measures of it, mixed with one of nitrous air, occupied the space of a little less than 2 measures. This portion of air was strongly impregnated with the peculiar smell of the cancerous matter. The heat continuing to increase, the water began to boil, and a large quantity of aqueous vapour arose; which, as soon as it came into contact with the common air, produced a white smoke. The smell now perceived was similar to that of fresh animal substances when boiled. The aqueous vapour in this part of the process was not mixed with any permanently elastic fluid.

When the greater part of the water was evaporated, the jar containing the first portion of air was removed, and the neck of the retort was introduced beneath an inverted vessel filled with mercury. Soon after this, a considerable quantity of air, having a fetid smell similar to that of burned bones, was extricated. This aërial fluid was mixed with a yellow empyreumatic oil. A portion of it being agitated with water was found to be partly imbibed by that fluid; and nitrated silver, dropped into the water thus impregnated, produced a reddish precipitate.

One measure of the air, obtained in the foregoing experiment, being mixed over mercury with an equal bulk of alkaline air, the volume of the mixture was found gradually to decrease; and, at the end of 3 hours, the air in the tube occupied the space of only 1 measure and $\frac{8}{10}$. An oily deposit was now made on the inner surface of the tube. At the expiration of 8 days, the interior surface of the tube was covered with slender films, which had a yellowish cast, and which were irregularly spread on it. The upper surface of the mercury within the tube was corroded; in some places it had a reddish burnished appearance; in others, it was changed into an ash-coloured powder, interspersed with brown spots. The tube was now removed from the mercury, and the air that remained in it had a strong fetid smell, resembling that of burnt bones.

It has been already observed, that before the water was entirely evaporated, the vapour had lost the odour of the cancerous matter, and had acquired that of

animal substances recently boiled. Hence it appears, that the matter on which the peculiar smell of cancerous ulcers depends, is a very volatile substance, for it escaped at the beginning of the process. It also appears that this volatile substance, which is probably the active principle in the matter of cancer, is not changed, by simple exposure to heat, into a permanently elastic fluid; for the air that escaped at the beginning of the process, though it smelled strongly of the cancerous matter, was found by Dr. Priestley's test to be as pure as common air; and it was evident, that the aqueous vapour which came over in the middle of the process was not mixed with any permanently elastic fluid; because, when this vapour was received in an inverted bottle filled with mercury, it was condensed into water, without any admixture of air. Indeed, if the odoriferous principle in the matter of cancer consist of volatile alkali combined with animal hepatic air, it could not be expected that it should acquire a permanently elastic form by simple exposure to heat; because when alkaline and animal hepatic air unite together, they form a non-elastic substance that condenses on the inner surface of the vessel in which they are mixed.

To discover whether other animal substances yield an aërial fluid, similar to that which was extricated in the foregoing experiment from the matter of cancer by means of heat, a portion of the flesh of the neck of a chicken was introduced into a small coated glass retort, which was gradually exposed to heat in a sand bath till it became red-hot. A thin phlegm, of a yellowish colour, first came over: this was soon succeeded by a yellow empyreumatic oil, and at the same time a permanently elastic fluid, having an odour resembling that of burnt feathers, began to be disengaged. A slip of paper, tinged with litmus and reddened by acetous acid, being held over this fluid, became blue. The neck of the retort was now introduced below an inverted jar filled with mercury, and à considerable quantity of air, together with a fetid empyreumatic oil, were received in the jar. This air was highly inflammable: it had a very fetid odour. When a bottle, containing a portion of it, was agitated with distilled water, nearly one-half of it was absorbed. The residue was inflammable, and burned first with a slight explosion, and afterwards with a blue lambent flame. A little nitrated silver being dropped into the water with which the air had been agitated, the mixture instantly acquired a reddish brown colour; after some time it became turbid, and a brown precipitate fell to the bottom. When 2 measures of the air, extricated in this experiment, were mixed with 1 of alkaline air, they occupied the space of a little more than 1 measure and a half. A 2d measure of alkaline air being added, and the airs being suffered to remain together for 3 days, at the end of that time the residue occupied the space of $2\frac{1}{8}$ measures. Soon after they were mixed, an oily fluid, of a pale colour, was deposited on the internal surface of the jar. At the end of the 3d day this substance had acquired a light olive colour. It was

collected in globules, irregularly distributed over the interior surface of the jar. These globules were nearly of a solid consistence. When the jar was removed from the mercury, the air contained in it at first smelled strongly of volatile alkali. After a little time the smell of the alkali disappeared, and the odour of empyreumatic oil was distinctly perceived. A small quantity of distilled water, which was now agitated in the jar, acquired a brown colour, but did not entirely dissolve the viscid substance that adhered to its surface. The water, thus coloured, was divided into 2 portions. To one of these was added a little strong vitriolic acid, by which the smell was exalted, and a slight effervescence was produced. Concentrated nitrous acid being added to the other portion, the smell and colour were destroyed, and a brisk effervescence took place.

When a portion of the solid substance that adhered to the interior surface of the jar was separated, it felt viscid and adhesive between the fingers, and smelled strongly of empyreumatic oil. A little spirit of wine being introduced into the jar, this viscid substance was dissolved; the spirit acquired a yellow colour and empyreumatic smell, and on adding to it distilled water the mixture became whitish and slightly turbid.

Dr. C. next examined the air extricated from putrid veal by distillation. A portion of the latter substance being introduced into a coated glass retort was exposed to a red heat, and the air disengaged was received in a jar over mercury. This aërial fluid was found to possess nearly the same properties with that which was obtained in the preceding experiments. It was very inflammable; about $\frac{1}{5}$ of it was soluble in distilled water. The water, thus impregnated, became turbid on the addition of nitrated silver, and a brown precipitate fell to the bottom. To another portion of distilled water saturated with this fluid, dephlogisticated marine acid being added, the fetid smell was destroyed, a brisk effervescence took place, and a whitish gelatinous substance was separated. This substance being evaporated to dryness, became black on the addition of concentrated vitriolic acid. When a quantity of the air obtained in the experiment was agitated with distilled water till no more was absorbed, the residue took fire on the application of an ignited body, and burned with a lambent flame. The air extricated from the putrid veal had less of the empyreumatic smell than that which was disengaged from fresh animal substances. Its odour indeed was nearly similar to that of animal substances in a state of putrefaction.

We learn from these experiments that the aërial fluids, which are extricated from fresh as well as from putrid animal substances by distillation, have nearly the same properties with that which is disengaged, by a similar process from the matter of cancer. Each of them appears to consist of 2 distinct fluids; one of which is soluble, and the other insoluble, in water. The portion that is insoluble burns with a lambent flame, and has all the characters of heavy inflammable

air; whereas the soluble part resembles the fluid which is extricated from cancerous matter by the vitriolic acid: it has a fetid odour, it decomposes nitrated silver, combines with caustic volatile alkali, and possesses many of the properties of common hepatic air.

There are several particulars however, in which the animal and common hepatic air materially differ from each other. Though they are both fetid, yet their odours are not exactly similar. When common hepatic air is decomposed by the concentrated nitrous or dephlogisticated marine acid, sulphur is separated; but when animal hepatic air is decomposed by these acids, a white flaky matter is disengaged which is evidently an animal substance, because it becomes black by the addition of concentrated vitriolic acid. Sulphur is also separated during the combustion of common hepatic with atmospherical air; but when the air from animal substances is burned with atmospherical air, no precipitation of sulphur takes place. Indeed, that animal hepatic air does not contain sulphur will be apparent from the following experiment. Equal parts of pure air and of air extricated from fresh beef by distillation, were fired by the electric shock in a strong glass tube over mercury. A little distilled water was then introduced through the mercury into the tube, and was agitated with the air which it contained. A portion of this water being filtered, and a small quantity of muriated barytes being dropped into it, the mixture remained perfectly transparent. Hence it appears, that the air extricated from fresh beef by distillation does not contain sulphur; for, if it had contained that substance, the sulphur, by its combustion with the pure air, would have been changed into the vitriolic acid, and the muriated barytes would have been decomposed.

The following experiments were made with a view more accurately to analyze the airs which are disengaged from animal substances by heat, and to determine the products resulting from the union of these fluids with pure air. About an ounce of the lean of fresh mutton was introduced into a small coated glass retort, and exposed to a red heat. The air extricated towards the end of the distillation was received over mercury; and soon after its production, being agitated with water, very nearly $\frac{1}{4}$ of it was absorbed. A similar experiment being made with the air disengaged towards the middle of the distillation, the part of it which was soluble in water was found to be to the part not soluble in that fluid, as 2 to 3. Having suffered a separate portion of the air disengaged towards the end of the distillation to remain over mercury for 7 hours, it was found gradually to diminish in bulk, and a fluid, which had the colour and the odour of a thin empyreumatic oil, was collected at the bottom of the jar. The air being now agitated with water, only $\frac{1}{8}$ of it was absorbed. Hence it appears, that a portion of the air, extricated from animal substances by heat, resembles a species of hepatic air which was first discovered by Mr. Kirwan, and which exists in an in-

termediate state between the aërial and the vapourous; this fluid not being permanently elastic like air, nor immediately condensed by cold like vapour, but gradually assuming the non-elastic form, in consequence probably of the tendency of its several parts to unite with each other. The air produced in the foregoing experiment rendered lime-water turbid; it therefore contained a quantity of fixed air; and towards the end of the distillation a little volatile alkaline air came over, agreeably to the observation of M. Berthollet: for when a portion of the air received during this part of the process was mixed with an equal quantity of marine acid air, a white vapour was produced, and a diminution of about $\frac{1}{15}$ of the whole took place.

Dr. C. endeavoured, by the following experiment, to ascertain the proportion of fixed air contained in the aerial fluid which is disengaged from the lean of animal substances by heat. A quantity of air, extricated from the lean of fresh mutton, was received over mercury in a large phial with a narrow neck. When the phial was a little more than half filled, the remaining portion of the mercury was displaced by introducing water that had been previously boiled. The phial being then closely corked, the air and water were briskly agitated together; and the liquor, thus impregnated with the soluble part of the animal air, was put into a proof, to the bottom of which heat was applied. By this means a portion of the air was again disengaged, which was received in a tube inverted over mercury. The process was continued till the liquor in the proof no longer rendered lime-water turbid. As the air received in the tube contained the fixed air that had been extricated from the liquor, together with a quantity of common air expelled from the proof, it was a 2d time agitated with water; and the exact measure of the fixed air was known by the portion which the water imbibed. The fixed air, thus ascertained, being compared with the entire quantity of air that had been originally absorbed, it appeared, that the former was to the latter in bulk as 1 to 4.' Therefore $\frac{1}{4}$ of the volume of the soluble part of animal air consists of fixed air, and the remaining $\frac{3}{4}$ of hepatic, mixed with a very small proportion of alkaline air.

It appeared from the experiment, that animal hepatic air, when it was absorbed by water, was not capable of being again disengaged by a heat which raised the water to the boiling temperature; for, after the fixed air was expelled, the liquor in the proof was made to boil nearly half an hour, but no permanently elastic fluid could be disengaged. The portion of the liquor which now remained had a faint yellow colour; it smelled strongly of animal hepatic air, and deposited a brown precipitate on the addition of nitrated silver. It appears therefore, that the soluble part of the air which is disengaged from the lean of animal substances by heat, consists of 3 distinct fluids; of alkaline air, fixed

air, and animal hepatic air. It seemed extremely probable, that these 3 aërial fluids, slowly combining together, formed the oily empyreumatic substance which was collected at the bottom of the jar, while the air was undergoing the diminution described above. This conclusion was confirmed by trials that were made with the empyreumatic oil that came over during the latter part of the distillation: for when it was examined by chemical tests, soon after it was obtained, it was found to contain fixed air, volatile alkali, and animal hepatic air.

Dr. C. next endeavoured to determine the products which result from the combustion of pure air, with animal air, or with the compound aërial fluid extricated from the lean of animal substances by heat. With this intention he exposed the lean of fresh mutton, in a small coated glass retort, to a red heat. The air which was received over mercury towards the end of the distillation, was divided into 2 separate portions; one of which was agitated with water till the soluble part was absorbed; the other was not agitated with that fluid. One measure of the former was introduced, over mercury, into a strong glass tube adapted for the purpose of firing aërial fluids by the electric shock. This was mixed with one measure and a half of pure air. The portion of the tube occupied by the mixture was 1 $\frac{8}{10}$ inch. A small shock being made to pass through it, a violent explosion took place, and the space occupied by the residue was $\frac{9}{10}$ of an inch. The height of the mercury in the tube, before the combustion, was 4.8 inches. After the airs were fired, its height was 5.1 inches. Allowance being made for the difference of expansion produced by this cause, it appeared that the volumes of the airs, before the combustion, and after it, were as 100 to 75 nearly. The residue being agitated with water, $\frac{6}{10}$ were absorbed; and the portion which was thus absorbed was found, by the precipitation it produced in lime-water, to be fixed air. Of the insoluble remainder, 5 parts being mixed with 5 of nitrous air, a diminution of 3 parts took place; whence it follows, that $\frac{1}{5}$ of the insoluble residue was pure air. The pure air used in this experiment had been previously agitated with water, to free it entirely from fixed air, and the inflammable air had undergone a similar agitation. It is therefore manifest that, by the combustion of the pure and inflammable air in the foregoing trial, fixed air was produced; the phlogisticated air, found in the residue, being that which was contained in the pure air before the inflammation took place.

Dr. C. next examined the products resulting from the combustion of pure air with that portion of the animal air which had not been previously agitated with water. One measure of this fluid, at the expiration of 3 quarters of an hour after it had been obtained, was mixed over mercury with one measure and a half of pure air, and fired by the electric shock. The portion of the tube occupied by the mixture, before the deflagration, was 1 inch and $\frac{15}{100}$; after the deflagra-

tion, it occupied the space of 1 inch and $\frac{1}{10}$. Being agitated with lime-water, very nearly $\frac{1}{4}$ was absorbed. A portion of the insoluble residue was exposed to a lighted taper, and burned with a faint blue flame.*

The dephlogisticated air used in this experiment had been previously agitated with water, to free it entirely from fixed air. It was the purest dephlogisticated air he had ever seen: for when 1 measure of it was mixed with 1 measure and $\frac{2}{10}$ of nitrous air, the residue occupied the space of only $\frac{1}{10}$ of a measure. From the foregoing trial it was evident, that $1\frac{1}{4}$ parts of pure air were insufficient to saturate one of the animal air that had not been previously agitated with water. The experiment was therefore repeated as follows. Two parts of pure air being mixed with 1 of animal air, occupied $\frac{8}{10}$ of an inch. The mixture being fired by the electrical shock, the residue stood at a little less than $\frac{4}{10}$. When this residue was agitated with lime-water, it was almost wholly absorbed. By a subsequent trial it was found, that nearly half the animal air used in this experiment was soluble in water. Hence it appears, that the quantity of pure air required to saturate the insoluble part of the animal air, is somewhat less than that required to saturate the compound fluid which had not been previously agitated with water. But the latter fluid has been shown to consist almost entirely of heavy inflammable, animal, hepatic, and fixed air; and as the last of these is already saturated with pure air, it is manifest that the above-mentioned difference must depend on the animal hepatic air. Whence it follows, that the latter contains a large portion of the inflammable principle. From the quantity of fixed air produced in the last of the preceding experiments, there is also the utmost reason to believe, that the basis of heavy inflammable forms one of the constituent parts of animal hepatic air. When equal parts of pure and animal air were burned together, a considerable increase of bulk almost invariably took place; and when the proportion of the animal was to that of pure air as 21 to 15, the bulk of the mixture was increased one half. The air that remained after the combustion in the last-mentioned experiments was inflammable: for a portion of it being introduced into a small phial, and exposed to a lighted candle, it first exploded, and then burned with a blue lambent flame.

Being desirous of learning the cause of the increase of bulk in the foregoing experiments, the following trials were made. Three measures of animal were mixed with 2 of pure air, and several strong electrical shocks were made to pass through the mixture; but it would not take fire. Half a measure of pure air

* When this experiment was first made, the residue did not appear to be inflammable. It had been tried by applying an inflamed slip of paper to the mouth of a phial which was filled with it; but, upon repeating the experiment, when the phial containing the residuary air was carried into a dark room, and an ignited wax taper was applied to its mouth, an evident inflammation took place. —Orig.

was then added' and the mixture being fired, its bulk was increased from .9 of an inch to 1.3 inch.

Three measures of this residuary air were then mixed with 3 of pure air, and fired by the electric shock. The bulk of the mixture was reduced from 1 inch to .56. This being agitated with lime-water, $\frac{2}{3}$ were absorbed, and the remainder consisted almost wholly of pure air. From these facts it seems probable, that animal hepatic air consists of a combination of heavy and light inflammable air; and that when it is fired with a quantity of pure air not sufficient to saturate it, a portion of the animal air is resolved into its elementary principles, in consequence of which its bulk is increased.

Dr. C. was next desirous of learning whether an increase of size would be produced by making the electric shock pass through a mixture of pure and alkaline air. Having first accidentally taken 2 or 3 small shocks through a little alkaline air, and not observing a sensible augmentation of bulk, he then mixed it with an equal volume of pure air; and, as he supposed that no decomposition had taken place, he was not apprehensive of an explosion. Contrary however to expectation, the airs, when the electric shock was made to pass through them, entered rapidly into a union with each other. The jar which he held loosely in his hand, as it was inverted over the mercury, was carried obliquely upwards with great violence. Having broken the stand of the prime-conductor in its passage, it forced its way through the cylinder of the electrical machine, which it shivered to a thousand pieces. Dr. C. afterwards repeated this experiment with a very strong apparatus, the jar being pressed down by a plate of iron, for the purpose of retaining it in its place. It appeared, that when the alkaline and pure air were immediately mixed together, and a small shock was made to pass through them, they would not take fire; but when 3 or 4 shocks were previously taken through the alkaline air, and the latter was afterwards mixed with an equal bulk of pure air, they exploded with great violence. The residue, having cooled to the temperature of the surrounding air, was reduced to half the original bulk of the mixture. Of this residue $\frac{1}{6}$ was undecomposed alkaline air. The remainder was phlogisticated air.

Of the products which result from the combustion of sulphureous hepatic with pure air.—The hepatic air employed in the following experiments was procured, agreeably to the method which Mr. Kirwan has recommended, by adding marine acid to an artificial combination of sulphur and iron. Three measures of the air thus obtained were mixed in a strong glass tube over mercury, with 4 of pure air, and fired by the electric shock.

The pure air was previously agitated with lime-water to free it from fixed air, and a portion of the hepatic air, having been likewise agitated with lime-water, was found not to occasion any precipitation in that fluid. The airs were re-

duced by the explosion to $\frac{1}{4}$ of their original bulk. The residue was then transferred over mercury into a slender graduated tube, and distilled water being admitted, $\frac{8}{10}$ were absorbed. To a portion of this water, when filtered, vitriolated silver was added, which instantly occasioned a copious precipitate. To a 2d portion was added muriated barytes, which occasioned a slight white precipitate not re-dissolvable in a large quantity of water; lime-water being added to a 3d portion, did not produce any sensible precipitation. From the last fact it does not follow, that no fixed air existed in the residue, because the marine acid, which it evidently contained, would dissolve the calcareous earth of the lime-water. As a great diminution however resulted from the combustion; and as it appeared from chemical tests, that the residue was mostly composed of marine and vitriolic acid airs, it is manifest that if any fixed air was produced, its quantity must have been very inconsiderable.

It has been already observed, that a slight precipitation took place on the addition of the muriated barytes. The precipitate was much more considerable when, on repeating the experiment, the residue after the explosion was not transferred into a graduated tube before the admission of the distilled water; but the latter was immediately introduced into the vessel in which the airs were fired. The reason of this difference is evident. The slight precipitate by the muriated barytes, in the first instance, depended on the existence of a small quantity of vitriolic acid in an aërial form, or in the state of volatile vitriolic acid, which was transferred together with the phlogisticated and marine acid air into the 2d tube; but the greater part of the vitriolic acid produced by the combustion adhered, in a fixed state, to the surface of the tube in which the airs were fired; and therefore, when the distilled water was immediately introduced into this tube, a copious precipitate was deposited on the addition of muriated barytes. Hence it appears, that when pure air and sulphureous hepatic air, obtained from artificial pyrites by the marine acid, are fired together in the above proportions, the products are fixed vitriolic acid, together with a small quantity of the volatile vitriolic and marine acids, in an aërial form. The residue, which the distilled water did not absorb, was the phlogisticated air that existed in the pure air before the combustion.

From subsequent trials it appeared, that when hepatic and pure air were fired in equal bulks, the residue had a strong odour of volatile vitriolic acid, and also contained a small proportion of undecomposed hepatic air. These facts seem to prove, that the conversion of sulphur into volatile or fixed vitriolic acid depends on the quantity of pure air with which it is supplied. The marine acid air, found in this experiment, did not appear to form one of the constituent principles of the hepatic air, but to be merely diffused through it; for it was almost wholly separated, by means of distilled water, from a different portion of the

same air, which was placed in a tube inverted over mercury; the water having a stronger attraction to the marine acid than to the hepatic air.

By the following experiment Dr. C. endeavoured to determine whether vitriolic acid be produced by the combustion of hepatic with atmospherical air. One measure of hepatic air, obtained from artificial pyrites, was mixed over mercury with about 6 measures of atmospherical air, and fired by the electric shock. A copious precipitation of sulphur took place, the remaining air was then agitated with distilled water, the latter was filtered, and muriated barytes was added, which produced a white precipitate not dissoluble in a large quantity of water. From this, and the foregoing experiment it appears, that when sulphureous hepatic is burned with atmospheric air, a part of the sulphur is changed into vitriolic acid, and the rest is precipitated; but when it is burned with a sufficient quantity of pure air, the sulphur is wholly converted into vitriolic acid. Agreeably to this conclusion, the odour of the volatile vitriolic acid constantly accompanies the combustion of hepatic with common air in open vessels; and when concentrated nitrous acid is added to water impregnated with hepatic air, the filtered liquor becomes turbid on the addition of muriated barytes.

The quantity of pure air required to saturate sulphureous hepatic air, does not appear to correspond with the supposition that the last of these fluids consists of sulphur dissolved in light inflammable air; for sulphur, in order to its complete saturation, requires only 1.43 times its weight of pure air; but light inflammable air requires for its saturation at least 6 times its weight of that fluid. The specific gravity of hepatic air, as determined by Mr. Kirwan, is nearly equal to that of pure air. If therefore $\frac{1}{6}$ of the weight of hepatic consisted of light inflammable air, that fluid would require for its saturation 2.26 times its bulk of pure air: for the portion of it which consisted of light inflammable air would require a quantity of pure air equal in bulk to the hepatic; and the remaining portion, consisting of sulphur, would require a quantity equal to 1.26 of the hepatic. The entire quantity of pure air would therefore be to that of the hepatic as 2.26 to 1. If the hepatic contained $\frac{1}{17}$ of its weight of light inflammable air, it would require for its saturation 1.64 of its bulk of pure air. But from the foregoing experiments it appears, that the quantity of pure air, necessary to saturate 1 measure of hepatic air, is only 1.33 measures. Hence it is probable that this fluid does not consist of sulphur dissolved in light inflammable air.

If we make allowance for the marine acid which was diffused through the hepatic air, it will be found, that the quantity of pure air required to saturate it, is nearly the same with that which would be required to change an equal weight of sulphur into vitriolic acid. Whence it may be inferred, agreeably to the opinion of Mr. Kirwan, that hepatic air is sulphur which has acquired an aërial form by the application of heat. This conclusion is, he thinks, confirmed by

the following experiment. A little pure sulphur was introduced into an inverted tube, which had been previously filled with mercury, and the flame of a candle was applied to the extremity of the tube. In a short time a permanently elastic fluid was produced, which was found to have all the characters of hepatic air. It is probable however, that some degree of moisture is necessary to the success of this experiment, because the quantity of hepatic air which was thus obtained was not very considerable.

It has been already shown, that an oily matter was produced by the union between fixed air, volatile alkali, and animal hepatic air. The following experiment proves, that a substance, which has very much the appearance of oil, is formed by the combination of sulphureous hepatic air with fixed air and volatile alkali. A quantity of impure hepatic air was obtained by adding vitriolic acid to common liver of sulphur. When this fluid was agitated with lime-water, it produced a copious precipitation. It therefore contained a considerable proportion of fixed air. One measure of it was now introduced into a slender graduated tube, inverted over mercury, and was mixed with an equal bulk of alkaline air. As soon as the airs came into contact with each other, a white cloud was produced, the mercury began gradually to rise in the tube, and at the end of 6 hours the air that remained occupied the space of only 1 measure and $\frac{1}{4}$. The surface of the mercury within the tube first became black, and a part of it afterwards acquired a red colour resembling cinnabar. In the course of the experiment, a yellowish oleaginous substance was deposited on the interior surface of the tube. This substance, in some parts of the surface, formed itself into globules; in others it was extended into ramifications, having the resemblance of trees in miniature, and it gradually assumed a deeper colour, till at length it acquired a greenish cast. The substance, thus obtained, had a very fetid odour: it appeared to have a near resemblance to an animal oil which had become green by putrefaction. It was however soluble in water, and the odour of the solution was increased by the vitriolic, and destroyed by the concentrated nitrous and dephlogisticated marine acids.

Mr. Cruikshank, who assisted in most of the foregoing experiments, and on whose accuracy he could place the greatest reliance, examined, in Dr. C.'s absence, the red and black powders formed by the action of the hepatic air on the surface of the mercury, and found them to be æthiops mineral, and cinnabar.

Of the air extricated from animal substances by putrefaction.—In the beginning of July, 1789, about 2 ounces of veal, slightly putrid, was introduced into a large phial, filled with distilled water, and inverted over a quantity of the same fluid. At the end of 3 days a few bubbles of air had appeared at the bottom of the phial; the water had acquired a light brown colour, and emitted a fetid smell. At the expiration of 7 days we could perceive that the quantity of air at

the bottom of the phial was manifestly increased, though its progress was very slow. The water, by the dissolution of a part of the veal, had now acquired the consistence of a thin mucus, its brown colour was somewhat deepened, and it emitted a highly fetid smell. A little nitrated silver being dropped into a portion of this water, previously filtered, a dark brown precipitate was immediately produced. Lime-water, mixed with another portion of it, occasioned an ash-coloured precipitate; and when concentrated nitrous acid was added to a 3d portion, the fetid smell was destroyed, a slight effervescence took place, and a yellow flaky matter was disengaged. At the end of 7 weeks, a quantity of air, amounting to $2\frac{1}{8}$ dram measures was collected in the phial. This air had a fetid odour. Being agitated with water, $\frac{6}{10}$ of it was absorbed. The residue extinguished flame.

Dr. C. next examined the air extricated from veal suffered to putrefy over mercury.

On July 28, 1789, 2 drams and 24 grains of the lean of fresh veal was introduced into a narrow jar, filled with mercury, and inverted over that fluid. At the end of 8 days the air, which was slowly extricated, had communicated a brown colour to the surface of the mercury. On Sept. 13, the quantity of air disengaged was a little more than 2 ounce measures. This fluid had a very fetid smell. Two separate portions of distilled water being saturated with it, the first, on the addition of nitrated silver, deposited a brown precipitate; and the last, when it was mixed with lime-water, produced a brownish ash-coloured cloud. A 3d portion of the air being strongly agitated with distilled water, was reduced to $\frac{1}{16}$ of its original bulk. The residue extinguished flame. The veal which had remained so long in contact with the mercury had not lost its firm texture. Its smell was putrid, but not very offensive.

The quantity of elastic fluid collected in this experiment was much greater than in the preceding one; because in the preceding experiment, though the putrefaction advanced more rapidly, yet the fixed and hepatic air were absorbed by the water nearly as fast as they were disengaged from the putrid substance. Hence it appears, that the aërial fluids, which are extricated from the mucular fibres of animals by putrefaction, consist of fixed and animal hepatic, mixed with a very small proportion of phlogisticated air.*

Of the effects produced by exposing fresh animal substances to atmospherical, hepatic, and pure air.—Two tubes, of nearly the same size, were inverted over mercury. Into one of these was introduced common air, and into the other an equal bulk of hepatic air, obtained from liver of sulphur by the vitriolic acid. Equal quantities of fresh veal, consisting of a mixture of muscular fibres and of

* It may be proper to remark, that I have obtained, by distillation from the green leaves of a cabbage, an aërial fluid, which, in most of its properties, resembles animal hepatic air.—Orig.

fat, and weighing each 1 dram, were then exposed to these airs. At the end of 3 days the piece that was in contact with the common air had not altered its colour or consistence, but smelled a little putrid. The colour of the fatty parts of the piece that was exposed to the hepatic air was changed to a dark green, the muscular fibres were cracked and shrivelled on the surface as if they had been seared with a hot iron, and the whole had acquired a soft consistence.

Similar trials were made with 2 pieces of fresh veal, one of which was exposed over mercury to common air, and the other to air extricated from putrid veal by distillation. The former in 3 days had not changed its appearance; the latter had become green round the edges, and was interspersed with green spots. The surface of the mercury in the jar which contained the last had acquired a brown colour; whereas that of the mercury in the jar which contained the common air was clear and bright. The pieces of veal were suffered to remain in this situation for 6 weeks. After a few days had expired, that which was exposed to the animal air did not appear to suffer any further change. Its colour, which in the course of a week had become brown, continued unaltered, and no dissolution took place. The air at the last was very fetid; it occasioned a copious precipitate in lime-water; it was highly inflammable, and burned with a blue lambent flame. On the contrary, the piece which was exposed to the common air, did not, as has been already observed, so soon lose its fibrous texture, nor so speedily acquire a dark colour, as that which was in contact with the animal air. But the progress of its putrefaction did not appear to stop at the end of a few days, as in the latter instance. It advanced slowly, and at the end of 6 weeks a considerable part of the muscular fibres had run down to a brown liquid. The air in which it was placed now occasioned a copious precipitation in lime-water, and the brown liquid was found to be impregnated with animal hepatic and fixed air; the existence of the latter being known by means of lime-water, and that of the former by its occasioning a dark precipitate in a solution of nitrated silver, as well as by its fetid odour, which was increased by the vitriolic, and destroyed by the concentrated nitrous and dephlogisticated marine acids.

The following experiment was made with a view to determine whether pure air accelerates the progress of putrefaction in animal substances. In the month of Dec. 1789, equal portions of pure and of common air were introduced into 2 equal jars over mercury, in each of which was placed about 2 drams of fresh beef. At the end of a week, the beef which was exposed to the pure air had become highly putrid; but very little change was produced in that which was exposed to the common air.

The facts which have been ascertained by the preceding experiments, appear to lead to the following conclusions respecting the process of putrefaction in the lean of animal substances. The muscular fibres of animals contain fixed and

phlogisticated air, the inflammable principle in the state of heavy and of light inflammable air, and a substance which, by means of heat or of putrefaction, is capable of being converted into animal hepatic air. When the muscular fibre, after the death of the animal, is exposed to the pure air of the atmosphere; the latter, by a superior attraction, combining with the heavy inflammable air, produces fixed air, and at the same time furnishes the quantity of heat necessary to the formation of animal hepatic air. The cohesion of the fibre being thus destroyed, the fixed, as well as the light inflammable and phlogisticated air, which enter into its composition, are disengaged, and the 2 latter fluids, uniting with each other, produce the volatile alkali. The alterations which take place in putrefaction are in most respects similar to those which arise from destructive distillation. By exposure to heat the fixed air of the animal fibre is extricated, hepatic air and volatile alkali are produced, and the inflammable principle, not coming into contact with the pure air of the atmosphere, is raised in the form of heavy inflammable air.

He has found, that the fetid odour of animal hepatic air is destroyed by mixing it with pure air, and suffering it to remain in contact with that fluid for several weeks. When it was placed in this situation, it acquired an odour which was not exactly similar to any that he had ever before perceived, but which bore some resemblance to that of inflammable air obtained by dissolving iron in spirit of vitriol. The peculiar smell of animal hepatic air is likewise destroyed by agitating it with vinegar, or with the concentrated vitriolic acid. But the fluids which most speedily produce this effect, are the concentrated nitrous and dephlogisticated marine acids; and these fluids are known to abound with pure air. It is therefore extremely probable, that this alteration depends on a union between the pure air of the latter substances and the animal hepatic air, or some of its constituent parts.

It appears from the experiments which have been recited above, that in cancerous and other malignant ulcers, the animal fibres undergo nearly the same changes which are produced in them by putrefaction, or by destructive distillation. The purulent matter prepared for the purpose of healing the ulcer is, in such cases, mixed with animal hepatic air and volatile alkali. The compound formed by the union of these substances, which may perhaps not improperly be termed hepatised ammonia, decomposes metallic salts, and acts on metals: for we have seen, that when it was placed in a jar over mercury for several days, the surface of the mercury acquired a black colour; and that it instantly occasioned a dark precipitate in a solution of nitrated silver. These facts seem to afford an explanation of the changes produced in metallic salts, when they are applied to malignant ulcers. The volatile alkali combines with the acid of the metallic salt, and the animal hepatic air revives the metal, either by imparting to it the inflam-

mable principle, or by uniting with the pure air which the calx is supposed to contain. The metal, thus revived, is probably in some cases again corroded by the hepatised ammonia, which communicates to it a black colour. Thus we may account for the dark incrustation frequently formed on the tongue and internal fauces, when venereal ulcers of the throat are washed with a solution of corrosive sublimate. And hence also the dark tinge which is frequently communicated by ill-conditioned ulcers to poultices made with a solution of sugar of lead. The action of the hepatised ammonia likewise explains the reason why the probes are frequently corroded when they are introduced into sinuous ulcers, or applied to the surfaces of carious bones. To the same cause it is probably owing, that polished metallic vessels are quickly tarnished, when they are exposed to the effluvia of putrid animal substances.

From the foregoing experiments it also appears, that animal hepatic air imparts to the fat of animals recently killed a green colour; that it renders the muscular fibres soft and flaccid, and increases the tendency to putrefaction. It is therefore a septic principle; and hence it is extremely probable, that the compound of this fluid with volatile alkali, which is found in the matter discharged by the open cancer, produces deleterious effects; for though the mischief in cancerous ulcers seems principally to depend on a morbid action of the vessels, whence the unhealthy state of the matter discharged by such ulcers is supposed to derive its origin, yet from the corrosion of the coats of the larger blood-vessels, and the obstructions in the contiguous glands, there can be little doubt that this matter aggravates the disease. The experiments recited above appear to prove, that the hepatised ammonia is the ingredient which communicates to the cancerous matter its putrid smell, its greater thinness, and in short, all the peculiar properties by which it differs from healthy pus.

From these considerations it was inferred, that a medicine which would decompose the hepatised ammonia, and destroy the fetor of the animal hepatic air, without at the same time increasing the morbid action of the vessels, would be productive of salutary effects. The nitrous acid does not destroy the fetor of hepatic air, unless it be highly concentrated; and in this state it is well known that it speedily corrodes animal substances. But the fetor of hepatic air quickly disappears when it is mixed with the dephlogisticated marine acid, even though the latter be so much diluted with water as to render it a very mild application. Dr. C. has found that this acid, diluted with thrice its weight of water, gives but little pain when it is applied to ulcers that are not very irritable; and in several cases of cancer it appeared to correct the fetor, and to produce a thicker and more healthy pus. It is proper however to remark, that other cases occurred in which it did not seem to be attended with the same salutary effects. Indeed some cancerous ulcers are so extremely irritable, that applications which are at

all of a stimulating nature cannot be ventured on with safety. And hence if the observations made on the efficacy of this acid as an external application, should be confirmed by future experience, it must be left to the judgment of the surgeon to determine both the degree of its dilution, and the cases in which it may be employed with advantage.

The dephlogisticated marine acid, as is generally known, has the power of destroying the colour, the smell, and perhaps the taste, of the greater part of animal and vegetable substances. We have seen that it corrects the fetor of putrid flesh. And he has found that, when it is poured in sufficient quantity on hemlock and opium, these narcotics speedily lose their sensible qualities. As it appears therefore to possess the power of correcting the vegetable, and probably many of the animal poisons, it seemed not unlikely that it might be useful as an internal medicine. Conceiving that its exhibition would be perfectly safe, Dr. C. once took 20 drops of it diluted with water. He soon afterwards however felt an obtuse pain, with a sense of constriction, in the stomach and bowels. This uneasiness, notwithstanding the use of emetics and laxatives, lasted for several days, but was at length removed by drinking water impregnated with sulphureous hepatic air. He afterwards found that the manganese, which had been used in the distillation of the acid, contained a small portion of lead.

Dr. Ingenhousz informed Dr. C. that a Dutchman of his acquaintance, some time ago, drank a considerable quantity of the dephlogisticated marine acid: the effects which it produced were so extremely violent, that he narrowly escaped with his life. If therefore this acid should hereafter be employed as an internal medicine, it would be necessary to prepare it by means of manganese that has been previously separated, by a chemical process, from the lead and the other metals with which that substance is usually contaminated.

XXIII. On the Satellites of the Planet Saturn, and the Rotation of its Ring on an Axis. By Wm. Herschel, LL.D., F. R. S. p. 427.

In Dr. H.'s last paper on the planet Saturn, the principal object of which was to give an immediate account of the most interesting phenomena that had occurred till the beginning of November, many things were left unnoticed for want of time to treat of them with sufficient accuracy; but having now before him the whole series of observations from July 18 till Dec. 25, 1789, he enters into a proper examination, assisted by such necessary calculations as then could not conveniently be made.

One of the principal motives which have induced Dr. H. to hasten this inquiry, is the frequent appearance of protuberant and lucid points on the arms of the ring of Saturn. He has mentioned before that such phenomena had been resolved by the situation of satellites that put on these appearances; but as his

observations were continued near 2 months afterwards, and as he had from them corrected the epochæ of the old satellites, and improved the tables of the new ones, he found that, besides many of these bright points which were completely accounted for by the calculated places of the satellites, there were also many more mentioned in his journal that would not accord with the situation of any of them.

The question then presented itself very naturally, what to make of these protuberant points? To admit 2 or 3 more satellites by way of solving such phenomena appeared too hazardous an hypothesis; especially as these lucid points, though some of them had a motion, did not seem willing to conform to the criterion he had before used of coming off the ring, and showing themselves as satellites. And yet a suspicion of at least one more satellite would often return; it was even considerably strengthened when he discovered, by means of re-calculating with great precision the whole series of observations, that in the beginning of the season there had been some few mistakes in the names of the satellites, when the observations of them were entered in the journal. In setting them right, which threw a great light on the revolution of the 6th, and more especially on that of the 7th, he found also, that some of the observations which were entered by the name of the 7th satellite could not belong to that, nor to any other known one. It remained therefore to be examined whether there might not be sufficient ground to suspect the existence of an 8th satellite.

In this situation of things, he thought it most advisable to draw out the whole series of observations in a paper, beginning at the 5th satellite, and thus gradually through the 4th, 3d, 2d, 1st, 6th, and 7th, to approach towards the centre of Saturn; that it might appear at last what observations were left unaccounted for. By this means also it may be seen clearly with how scrupulous an attention the identity of every satellite has been ascertained; and with a view to give the strongest satisfaction in this respect, at least one observation of each has been calculated for each night; and the place thus computed is set down in the notes, that it may be compared with the observed one. To facilitate this comparison, he delineated a scheme, in which the orbits of the satellites are drawn in their due proportion. A few words will explain the construction and use of this figure, which, notwithstanding its simplicity, is yet amply sufficient to ascertain the accuracy of every observation.

In each of the orbits, described round the centre of Saturn, at their proportional distances, by way of marking them, is placed the satellite to which it belongs, as it appeared to be situated the 18th of October, 1789; also a graduated circle, the use of which is to find, by means of the tables, the apparent place of a satellite for any given time; or, the apparent situation of the same satellite being given, its real Saturnicentric place may be deduced from it. In

the centre of the scheme of course is the planet Saturn, and its ring, expressed by a line which represents the direction of its ansæ; or the ring itself, as it appeared in the telescopes during the months of July, August, September, October, and November, 1789. The 5 lines which are carried on parallel to each other serve to convey the measure of the planet, and its ring, to the orbits of the satellites, as will be seen in several instances that occur hereafter. The graduated circle is divided into degrees, and begins to count from that part of every satellite's orbit beyond the planet, which is intercepted by a plane passing from the eye of the observer, at rectangles to the ring, through the centre of Saturn. Hence it follows, that the point of zero, or 360°, is the same with the geocentric place of the planet in those 4 parts of the orbit of the satellite where the eye is in the plane of the ring, and where it appears the most open; and that, in other places, it may be had by solving one spherical triangle. This is to be understood as relating only to the inner satellites; the 5th, or outermost, requiring a different reduction, on account of its deviation from the plane of the ring. But Dr. H. is inclined to believe, that the surest way of observing the 5th, is to trust only to measures, taken with micrometers which give the distance and angle of position, except in such cases when the eye is nearly in the plane of this satellite's orbit, where the different reductions may be neglected, without bringing on any considerable inaccuracies.

The calculations of the places of all the satellites have been made according to tables which are given at the end of this paper. Their form being very simple, Dr. H. thought it not amiss to communicate them, for the use of those who may wish to enter into a more particular examination of the followiug observations; or to follow the satellites in their orbits at any future time. Dr. H. has deduced the epochæ of all the 7 satellites from his own observations, and they will be found to differ considerably from those given by De la Lande, in the Connoissance des Temps for 1791. But he has not attempted to extend them further than a few years backwards or forwards, as he is not in possession of any observations that could authorize him to undertake such a work. On the contrary he is well convinced, that no tables will give the situation of the satellites accurately, till we have at least established the dimensions of their elliptical orbits, and the motion as well as the situation of their aphelia. The epochæ for 1789 therefore must be considered not as mean ones, but such as respect the orbits of these satellites in their situation during the time of the following observations; and the 2 preceding and 2 following years must be already a little affected with those errors which are the necessary consequence of our not knowing the required elements. Dr. H. flatters himself however, that the observations, which are delivered in this paper, will serve as a beginning to a proper foundation for investigating them. The many conjunctions between the satellites, for in-

stance, will undoubtedly throw some light on the situation and excentricity of their orbits; as it will be found, that the calculated places of these conjunctions require elliptical motions to bring the satellites to such appearances, which, in circular orbits, could not so accurately have taken place. Nor can we ascribe the disagreements to the fault of the observations, since a very few minutes will suffice to determine the time of a conjunction, which never lasts long. For this reason also, he carefully avoided deducing the epochæ from conjunctions, even with the 6th satellite, which moves so rapidly, that at first sight we might think those situations favourable.

The mean motion of the 5 old satellites being sufficiently accurate for the present purpose, Dr. H. has taken from the above-mentioned tables of De la Lande; but those of the 6th and 7th, of course, are the result of his own observations. The geocentric place of Saturn, whose complement is to be added, in order to reduce the Saturnicentric situation of the satellites to the apparent one, he has taken from the nautical almanac to the nearest minute; and, as he had always confined himself to a literal transcription of the observations from the original journal, all the memorandums which are necessary either to explain them, or to correct mistakes in the names of the satellites, are thrown into notes, that there may be no interruption in the succession of the observations. Dr. H. then gives a long series of the observations of all the satellites, as copied from his original journal, with all the minute particulars, of course not necessary to be re-printed here.

Dr. H. then resumes his discourse. From the observations on the 7 satellites of Saturn above-mentioned, and closely compared with their calculated places, it appears evidently that the revolutions of these satellites are so well ascertained, that we may, without hesitation, determine that no phenomenon on the ring of Saturn, in the shape of lucid spot, protuberant point, or latent satellite, can be occasioned by any of them, when, on computation, we find that the place of the satellite differs from that where such appearances were observed. In consequence of this deduction, he found that the observations could not be explained by any of the known satellites; it remained therefore to be examined, to what cause to ascribe the appearance of such lucid spots.

The first idea that occurred was that of another satellite, still closer to the ring than the 7th; and if a revolution, slower than about 15 hours and a quarter, could have been found, which would have taken in the most material places in which bright spots were seen, he should have continued of opinion that an 8th satellite, exterior to the ring, did exist, notwithstanding more observations had been wanting to put the matter out of all doubt. But this being impracticable, he examined, in the next place, what would be the result if these supposed satellites, or protuberant points, were attached to the plane or edge of the ring.

As observations, carefully made, should always take the lead of theories, Dr. H. will not be concerned if such lucid spots as he is now going to admit, should seem to contradict what has been said in his last paper, concerning the idea of inequalities, or protuberant points. We may however remark, that a lucid, and apparently protuberant point, may exist without any great inequality in the ring. A vivid light, for instance, will seem to project greatly beyond the limits of the body on which it is placed. If therefore the luminous places on the ring should be such as proceed from very bright reflecting regions, or, which is more probable, owe their existence to the more fluctuating causes of inherent fires acting with great violence, we need not imagine the ring of Saturn to be very uneven or distorted, in order to present us with such appearances as will be related. In this sense of the word, then, we may still oppose the idea of protuberant points, such as would denote immense mountains of elevated surface.

On comparing together several observations, a few trials show that the brightest nd best observed spot agrees to a revolution of $10^h\ 32^m\ 15^s.4$; and, calculating its distance from the centre of Saturn, on a supposition of its being a satellite, we find it $17''.227$, which brings it on the ring. It is therefore certain, that unless we should imagine the ring to be sufficiently fluid to permit a satellite to revolve in it, or suppose a notch, groove, or division in the ring, to suffer the satellite to pass along, we ought to admit a revolution of the ring itself. The density of the ring indeed may be supposed to be very inconsiderable by those who imagine its light to be rather the effect of some shining fluid, like an aurora borealis, than a reflection from some permanent substance; but its disapparition in general, and in the telescopes its faintness when turned edgeways, are in no manner favourable to this idea. When we add also, that this ring casts a deep shadow on the planet, is very sharply defined both in its outer and inner edge, and in brightness exceeds the planet itself, it seems to be almost proved, that its consistence cannot be less than that of the body of Saturn; and that consequently, no degree of fluidity can be admitted sufficient to permit a revolving body to keep in motion for any considerable time.

A groove might afford a passage, especially as on a former occasion we have already considered the idea of a divided ring. A circumstance also which seems rather to favour this idea is, that in some observations a bright spot has been seen to project equally on both sides, as the satellites have been observed to do when they passed behind the ring. But, on the other hand, we ought to consider that the spot has often been observed very near the end of the arms of Saturn's ring, and that the calculated distance is consequently a little too small for such appearances, and ought to be 19 or 20″ at least. We should also attend to the size of the spot, which seems to be variable; for it is hardly to be imagined that a satellite, brighter than the 6th, and which could be seen with

the moon nearly at the full, should so often escape our notice in its frequent revolutions, unless it varied much in its apparent brightness.

To this we must add another argument drawn from the number of lucid spots, which will not agree with the motion of one satellite only; whereas, by admitting a revolution of the ring itself, in $10^h\ 32^m\ 15^s.4$; and supposing all the spots to adhere to the ring, and to share in the same periodical return, provided they last long enough to be seen many times, we shall be able to give an easy solution to all the remaining observations.

For instance, let α, β, γ, δ, ϵ, represent five spots on the ring of Saturn, situated as in fig. 5, pl. 8; where the ring is supposed to be divided into 360 degrees, and the spot α placed at $271^\circ.5$; β at $70^\circ.2$; γ at $183^\circ.0$; δ at $142^\circ.5$; and ϵ at $358^\circ.6$. Then will the ring, with the spots thus placed, serve as an epocha for the year 1789; by which, with the assistance of a table constructed on the before mentioned period of the rotation of the ring, we may calculate their situation for any required time; and to render this calculation perfectly convenient, a table is given, ready prepared for the purpose, at the end of the other tables. Dr. H. then gives another set of observations, which had all been previously calculated by the tables of such of the 7 satellites as were not already in view, and had been found to belong to neither of them; but in the notes that are given with them they have been again calculated by the table of the rotation of the ring for every time they were observed, on a supposition of their being spots adhering to it. He then adds:

The great accordance between the observed places of these spots and the calculated ones, seems to establish the rotation of the ring of Saturn on an axis, so as hardly to leave any doubt on the subject. The time of it, we have already seen, is $10^h\ 32^m\ 15^s.4$. It may be objected, that many of the observations are such as would also agree with other assignable periods, especially when the number of spots is so considerable as 5; but the most material observations, which are those on the spot α, setting aside all the rest, seem alone to amount to a proof not only of a rotation of the ring, but of the time in which it is performed.

It may be expected, that having now sufficiently examined the whole series of observations of the last new satellites, we can give their periodical times and distances more accurately than before. The times indeed are full as well ascertained as we can expect to have them: for on calculating 6 satellites by his tables back to Aug. $19^d\ 12^h\ 19^m\ 56^s$, 1787, we find their places $341^\circ.1$ the 5th; $10^\circ.6$ the 4th; $211^\circ.1$ the 3d; $158^\circ.9$ the 2d; $80^\circ.2$ the 1st; and $288^\circ.8$ the 6th. And Dr. H's journal contains the fullest assurance that they were thus situated at the time for which this calculation is made. We may therefore fix the period of the 6th at $1^d\ 8^h\ 53^m\ 8^s.9$. The 7th satellite can only be traced back as far as the 8th of Sept. 1789; so that its revolution will require at least

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another season to come to some degree of accuracy; till when he states it at $22^h\ 37^m\ 22^s.9$.

The distance of these satellites, deduced from calculation, depends entirely on the time and distance of the 4th, which is the satellite that has been used. In order to obtain more accuracy in these elements, he applied himself to measuring the distance of the 4th satellite in those moments which were most favourable for the purpose. It is well known that this subject, on account of the quantity of matter in Saturn, to be deduced from the periodical times and distances of the satellites, is of considerable importance to astronomers; he therefore defers a full investigation of it till he can have an opportunity of calculating a great number of measures, not only of the 4th and 5th, but also of the other satellites which he had already by him, and still intends next season to take. Mean while, having brought the measures of the 30th of November, which seem to be very good ones, to the mean distance of Saturn from the sun, he finds they give the distance of the 4th satellite from Saturn 3′ 8″.918. In reducing these measures to the mean distance, he used the new tables of De Lambre for Saturn, and Mayer's for the sun. Admitting therefore the above quantity as the distance, and $15^d\ 22^h\ 41^m\ 13^s.4$ as the period of the 4th satellite, we compute that the distance of the 6th from the centre of Saturn is 36″.7889; and that of the 7th, 28″.6689.

Tables for the seven satellites of Saturn.

Epochs of the mean longitude of the satellites.

Years.	5 sat.	4 sat.	3 sat.	2 sat.	1 sat.	6 sat.	7 sat.
	°	°	°	°	°	°	°
1787	335.91	149.16	87.21	272.18	176.46	269.31	307.07
1788	196.84	132.41	93.86	173.95	131.91	307.48	65.02
1789	53.23	93.09	20.82	304.19	256.66	82.92	161.00
1790	269.63	53.77	307.78	74.43	21.41	218.36	256.98
1791	126.02	14.45	234.74	204.68	146.16	353.81	352.97

Saturnicentric motion of the satellites in months.

Months.	5th.	4th.	3d.	2d.	1st.	6th.	7th.
	°	°	°	°	°	°	°
January,	000.00	000.00	000.00	000.00	000.00	000.00	000.00
February,	140.68	339.89	310.40	117.58	151.64	224.54	320.81
March,..	267.75	252.05	21.73	200.56	91.18	20.91	215.73
April,...	48.43	231.95	332.13	318.14	242.81	245.45	176.54
May,...	184.57	189.26	202.84	304.19	203.75	207.27	115.39
June,...	325.25	169.16	153.24	61.77	355.39	71.81	76.20
July,....	101.39	126.47	23.94	47.82	316.33	33.63	15.05
August,	242.07	106.37	334.34	165.40	107.96	258.17	335.86
Septem.	22.75	86.26	284.74	282.98	259.60	122.72	296.67
October,	158.89	43.58	155.45	269.03	220.54	84.54	235.52
Novem.	299.57	23.47	105.85	26.61	12.17	309.08	196.33
Decem.	75.71	340.78	336.56	12.66	333.11	27.090	135.17

In the months January and February of a bissextile year substract 1 from the number of days given.

Motion of the satellites in days.

Days	5th.	4th.	3d.	2d.	1st.	6th.	7th.
1	4°.54	22°.58	79°.69	131°.53	190°.70	262°.73	21°.96
2	9.08	45.15	159.38	263.07	21.40	165.45	43.92
3	13.61	67.73	239.07	34.60	212.09	68.18	65.88
4	18.15	90.31	318.76	166.14	42.79	330.91	87.85
5	22.69	112.89	38.45	297.67	233.49	233.64	109.81
6	27.23	135.46	118.14	69.21	64.19	136.36	131.77
7	31.77	158.04	197.83	200.74	254.89	39.09	153.73
8	36.30	180.62	277.52	332.28	85.58	301.82	175.69
9	40.84	203.19	357.21	103.81	276.28	204.55	197.65
10	45.38	225.77	76.90	235.35	106.98	107.27	219.62
11	49.92	248.35	156.59	6.88	297.68	10.00	241.58
12	54.46	270.93	236.28	138.42	128.38	272.73	263.54
13	58.99	293.50	315.97	269.95	319.07	175.45	285.50
14	63.53	316.08	35.66	41.49	149.77	78.18	307.46
15	68.07	338.66	115.35	173.02	340.47	340.91	329.42
16	72.61	1.24	195.04	304.56	171.17	243.64	351.39
17	77.15	23.81	274.74	76.09	1.87	146.36	13.35
18	81.69	46.39	354.43	207.63	192.56	49.09	35.31
19	86.22	68.97	74.12	339.16	23.26	311.82	57.27
20	90.76	91.54	153.81	110.70	213.96	214.54	79.23
21	95.30	114.12	233.50	242.23	44.66	117.27	101.19
22	99.84	136.70	313.19	13.77	235.35	20.00	123.16
23	104.38	159.28	32.88	145.30	66.05	282.73	145.12
24	108.91	181.85	112.57	276.84	256.75	185.45	167.08
25	113.45	204.43	192.26	48.37	87.45	88.18	189.04
26	117.99	227.01	271.95	179.91	278.15	350.91	211.00
27	122.53	249.58	351.64	311.44	108.84	253.64	232.06
28	127.07	272.16	71.33	82.98	299.54	156.36	254.92
29	131.60	294.74	151.02	214.51	130.24	59.09	276.89
30	136.14	317.32	230.71	346.05	320.94	321.82	298.85
31	140.68	339.89	310.40	117.58	151.64	224.54	320.81

Motion of the satellites in hours.

Hours	5th.	4th.	3d.	2d.	1st.	6th.	7th.
1	0°.19	0°.94	3°.32	5°.48	7°.95	10°.95	15°.92
2	0.38	1.88	6.64	10.96	15.89	21.89	31.83
3	0.57	2.82	9.96	16.44	23.84	32.84	47.75
4	0.76	3.76	13.28	21.92	31.78	43.79	63.66
5	0.95	4.70	16.60	27.40	39.73	54.73	79.58
6	1.13	5.64	19.92	32.88	47.67	65.68	95.49
7	1.32	6.58	23.24	38.36	55.62	76.63	111.41
8	1.51	7.53	26.56	43.84	63.57	87.58	127.32
9	1.70	8.47	29.88	49.33	71.51	98.52	143.24
10	1.89	9.41	33.20	54.81	79.46	109.47	159.15
11	2.08	10.35	36.52	60.29	87.40	120.42	175.07
12	2.27	11.29	39.84	65.77	95.35	131.36	190.98
13	2.46	12.23	43.17	71.25	103.29	142.31	206.90
14	2.65	13.17	46.49	76.73	111.24	153.26	222.81
15	2.84	14.11	49.81	82.21	119.19	164.20	238.73
16	3.03	15.05	53.13	87.69	127.13	175.15	254.64
17	3.21	15.99	56.45	93.17	135.08	186.10	270.56
18	3.40	16.93	59.77	98.65	143.02	197.05	286.47
19	3.59	17.87	63.09	104.13	150.97	207.99	302.39
20	3.78	18.81	66.41	109.61	158.91	218.94	318.30
21	3.97	19.75	69.73	115.09	166.86	229.89	334.22
22	4.16	20.70	73.05	120.57	174.81	240.83	350.13
23	4.35	21.64	76.37	126.05	182.75	251.78	6.05
24	4.54	22.58	79.69	131.53	190.70	262.73	21.96

Motion of the satellites in minutes.

Min.	5th.	4th.	3d.	2d.	1st.	6th.	7th.
1	0°.00	0°.02	0°.06	0°.09	0°.13	0°.18	0°.27
2	0 .01	0 .03	0 .11	0 .18	0 .26	0 .36	0 .53
3	0 .01	0 .05	0 .17	0 .27	0 .40	0 .55	0 .80
4	0 .01	0 .06	0 .22	0 .37	0 .53	0 .73	1 .06
5	0 .02	0 .08	0 .28	0 .46	0 66	0 .91	1 .33
6	0 .02	0 .09	0 .33	0 .55	0 .79	1 .09	1 .59
7	0 .02	0 .11	0 .39	0 .64	0 .93	1 .28	1 .86
8	0 .03	0 .13	0 .44	0 .73	1 .06	1 .46	2 .12
9	0 .03	0 .14	0 .50	0 .82	1 .19	1 .64	2 .39
10	0 .03	0 .16	0 .55	0 .91	1 .32	1 .82	2 .65
11	0 .04	0 .17	0 .61	1 .00	1 .46	2 .01	2 .92
12	0 .04	0 .19	0 .66	1 .10	1 .59	2 .19	3 .18
13	0 .04	0 .20	0 .72	1 .19	1 .72	2 .37	3 .45
14	0 .05	0 .22	0 .77	1 .28	1 .85	2 .55	3 .71
15	0 .05	0 .24	0 .83	1 .37	1 .99	2 .74	3 .98
16	0 .05	0 .25	0 .89	1 .46	2 .12	2 .92	4 .24
17	0 .06	0 .27	0 .94	1 .55	2 .25	3 .10	4 .51
18	0 .06	0 .28	1 .00	1 .64	2 .38	3 .28	4 .78
19	0 .06	0 .30	1 .05	1 .73	2 .52	3 .47	5 .04
20	0 .07	0 .31	1 .11	1 .83	2 .65	3 .65	5 .31
21	0 .07	0 .33	1 .16	1 .92	2 .78	3 .83	5 .57
22	0 .07	0 .34	1 .22	2 .01	2 .91	4 .01	5 .84
23	0 .08	0 .36	1 .27	2 .10	3 .05	4 .20	6 .10
24	0 .08	0 .38	1 .33	2 .19	3 .18	4 .38	6 .37
25	0 .08	0 .39	1 .38	2 .28	3 .31	4 .56	6 .63
26	0 .09	0 .41	1 .44	2 .37	3 .44	4 .74	6 .90
27	0 .09	0 .42	1 .49	2 .47	3 .57	4 .93	7 .16
28	0 .09	0 .44	1 .55	2 .56	3 .71	5 .11	7 .43
29	0 .10	0 .45	1 .60	2 .65	3 .84	5 .29	7 .69
30	0 .10	0 .47	1 .66	2 .74	3 .97	5 .47	7 .96
31	0 .10	0 .49	1 .72	2 .83	4 .10	5 .66	8 .22
32	0 .11	0 .50	1 .77	2 .92	4 .24	5 .84	8 .49
33	0 .11	0 .52	1 .83	3 .01	4 .37	6 .02	8 .75
34	0 .11	0 .53	1 .88	3 .10	4 .50	6 .20	9 .02
35	0 .12	0 .55	1 .94	3 .20	4 .63	6 .39	9 .29
36	0 .12	0 .56	1 .99	3 .29	4 .77	6 .57	9 .55
37	0 .12	0 .58	2 .05	3 .38	4 .90	6 .75	9 .82
38	0 .13	0 .60	2 .10	3 .47	5 .03	6 .93	10 .08
39	0 .13	0 .61	2 .16	3 .56	5 .16	7 .12	10 .35
40	0 .13	0 .63	2 .21	3 .65	5 .30	7 .30	10 .61
41	0 .14	0 .64	2 .27	3 .74	5 .43	7 .48	10 .88
42	0 .14	0 .66	2 .32	3 .83	5 .56	7 .66	11 .14
43	0 .14	0 .67	2 .38	3 .93	5 .69	7 .85	11 .41
44	0 .15	0 .69	2 .43	4 .02	5 .83	8 .03	11 .67
45	0 .15	0 .71	2 .49	4 .11	5 .96	8 .21	11 .94
46	0 .15	0 .72	2 .55	4 .20	6 .09	8 .39	12 .20
47	0 .16	0 .74	2 .60	4 .29	6 .22	8 .58	12 .47
48	0 .16	0 .75	2 .66	4 .38	6 .36	8 .76	12 .73
49	0 .16	0 .77	2 .71	4 .47	6 .49	8 .94	13 .00
50	0 .17	0 .78	2 .77	4 .57	6 .62	9 .12	13 .27
51	0 .17	0 .80	2 .82	4 .66	6 .75	9 .30	13 .53
52	0 .17	0 .82	2 .88	4 .75	6 .88	9 .49	13 .80
53	0 .17	0 .83	2 .93	4 .84	7 .02	9 .67	14 .06
54	0 .18	0 .85	2 .99	4 .93	7 .15	9 .85	14 .33
55	0 .18	0 .86	3 .04	5 .02	7 .28	10 .03	14 .59
56	0 .18	0 .88	3 .10	5 .11	7 .41	10 .22	14 .86
57	0 .19	0 .89	3 .15	5 .20	7 .55	10 .40	15 .12
58	0 .19	0 .91	3 .21	5 .30	7 .68	10 .58	15 .39
59	0 .19	0 .93	3 .27	5 .39	7 .81	10 .76	15 .65
60	0 .20	0 .94	3 .32	5 .48	7 .94	10 .95	15 .92

Table of the rotation of the ring of Saturn.

Epochs for 1789.		Motion of the spots in days, hours, and minutes.							
		Days	Degrees.	Hou.	Degrees.	Min.	Degrees.	Min.	Degrees.
Spot α	271.5	1	99.92	1	34.16	1	0.57	31	17.65
β	183.0	2	199.84	2	68.33	2	1.14	32	18.22
γ	70.2	3	299.76	3	102.49	3	1.71	33	18.79
δ	142.5	4	39.68	4	136.65	4	2.28	34	19.36
ε	358.6	5	139.60	5	170.81	5	2.85	35	19.93
		6	239.52	6	204.98	6	3.42	36	20.50
Motion of the spots in Months.		7	339.44	7	239.14	7	3.99	37	21.07
		8	79.36	8	273.30	8	4.56	38	21.64
Months.	Deg.	9	179.28	9	307.46	9	5.12	39	22.21
		10	279.20	10	341.63	10	5.69	40	22.78
January...	000.00	11	19.12	11	15.79	11	6.26	41	23.35
February..	217.52	12	119.04	12	49.95	12	6.83	42	23.91
March....	135.28	13	218.96	13	84.11	13	7.40	43	24.48
April.....	352.80	14	318.88	14	118.28	14	7.97	44	25.05
May	110.40	15	58.80	15	152.44	15	8.54	45	25.62
June	327.92	16	158.72	16	186.60	16	9.11	46	26.19
July......	85.52	17	258.64	17	220.76	17	9.68	47	26.76
August....	303.04	18	358.56	18	254.93	18	10.25	48	27.33
September	160.56	19	98.48	19	289.09	19	10.82	49	27.90
October...	278.16	20	198.40	20	323.25	20	11.39	50	28.47
November	135.68	21	298.32	21	357.41	21	11.96	51	29.04
December	253.28	22	38.24	22	31.59	22	12.53	52	29.61
		23	138.16	23	65.75	23	13.10	53	30.18
		24	238.08	24	99.91	24	13.67	54	30.75
		25	338.00			25	14.24	55	31.32
		26	77.92			26	14.80	56	31.89
		27	177.84			27	15.37	57	32.46
		28	277.76			28	15.94	58	33.03
		29	17.68			29	16.51	59	33.59
		30	117.60			30	17.08	60	34.16
		31	217.52						

Example of the use of the tables.—Let it be required to calculate the apparent place of the 7 satellites for 1789, Oct. 18, $7^h\ 51^m\ 54^s$, to the nearest minute of time and to 10ths of a degree.

	5th.	4th.	3d.	2d.	1st.	6th.	7th.
1789	53.23	93.09	20.82	304.19	256.66	82.92	161.00
Oct.	158.89	43.58	155.45	269.03	220.54	84.54	235.52
18	81.69	46.39	354.43	207.63	192.56	49.09	35.31
7	1.32	6.58	23.24	38.36	55.62	76.63	111.41
52	0.17	0.82	2.88	4.75	6.88	9.49	13.80
* ♄	12.58	12.58	12.58	12.58	12.58	12.58	12.58
	307.9	203.0	209.4	116.5	24.8	315.3	209.6

* The quantity marked ♄ 12°.58, which is applied to every one of the satellites, is the complement of 11^s 17° 25′, or geocentric place of Saturn, taken from the Nautical Almanac, for midnight of the required day, and to the nearest minute, which is sufficiently exact. This complement, or 12° 35′ in conformity with the tables, is reduced to decimals of a degree 12°.58.—Orig.

The situation of the spot α calculated for July 28, $13^h\ 53^m\ 39^s$; β for Sept. 16, $7^h\ 45^m\ 48^s$; ϵ for Nov. 2, $7^h\ 15^m\ 58^s$.

1789, α	271.5	β	183.0	ϵ	358.6
July	85.52	Sept.	160.56	Nov.	135.68
28	277.76	16	158.72	2	199.84
13	84.11	7	239.14	7	239.14
54	30.75	46	26.19	16	9.11
♄	7.20	♄	10.45	♄	13.18
	36.8		58.1		235.6

*XXIV. On Spherical Motion. By the Rev. Charles Wildbore.** p. 496.

This paper, which has cost me much pains in patient investigation, says Mr. W. is occasioned by that of Mr. Landen, in the Philos. Trans. vol. 75. I am no stranger to this gentleman's great judgment and abilities in these abstruse speculations, but have a very high opinion of both; yet I could not but think it strange, that two such mathematicians as M. D'Alembert and M. L. Euler should both follow each other on the same subject, both agree, and still not be right. I therefore resolved to try to dive to the bottom of their solutions, which those who are acquainted with the subject know to be no light task; and, if possible, to give the solution, independent of the perplexing consideration of a momentary axis changing its place both in the body and in absolute space every instant; and which I consider as not absolutely essential to the determination of the body's motion. But finding that I could not thus so readily show the agreement or disagreement of my conclusions with those of the gentlemen who have preceded me in this inquiry; I have also added the investigation of the properties of this axis. And I suppose it will be found that I have added many properties unknown before, or at least unnoticed by any of them.

M. Landen's very important discovery, that every body, be its form ever so irregular, will revolve in the same manner as if its mass were equally divided and placed in the 8 angles, or disposed in the 8 octants of a regular parallelopipedon, whose moments of inertia round its 3 permanent axes are the same as those of the body, serves admirably to shorten the investigation, and render the solution

* Mr. Wildbore died Oct. 30, 1802, being 65 years of age, at Broughton Sulney, Nottinghamshire, in which village he had been pastor more than 30 years. He commendably raised himself from a low origin, by his industry and natural talents, to an eminent rank in mathematics and classical learning. Before his care of the parish of Broughton, he kept an academy for young gentlemen several years, at Bingham, in the same county. Though an eminent philosopher and mathematician, Mr. W. never favoured the world with any separate publication of his own; but commonly amused himself in various periodical publications; as in Martin's Miscellaneous Correspondence, the Magazines, the Ladies' and Gentleman's Diaries, the Monthly Review, &c. After he became compiler of the Gentleman's Diary, in 1780, his writings in that annual little work were given under the signature Eumenes; and from that time, till his death, under that of Amicus in the Ladies' Diary.

perspicuous. I have therefore here taken its truth for granted, because it is also exactly agreeable to the solutions of the other gentlemen, and saves the trouble of repeating what they have done before. I have also shown wherein, and why, his solution differs from theirs, and proved, as I think, undeniably, in what respects it is defective.

That the inertia, or, as M. Euler calls it, the momentum of inertia, is equal to the fluent or sum of every particle of the body drawn into the square of its distance from the axis of motion; and the determination of the 3 permanent axes, or the demonstration that there are, at least 3 such axes in every body, round any one of which, if it revolved, the velocity would be for ever uniform, I have also taken for granted, because these things have been proved before, and all the gentlemen are agreed in them. Difficulties that occurred I have not concealed, but shown how to obviate, and endeavoured to place the truth in as clear a light as possible; which to discover is my wish, or to welcome it by whomsoever found.

Mr. W. divides his work into several propositions, which he demonstrates in an intricate algebraical method.

Prop. 1. While a globe, whose centre is at rest, revolves with a given velocity about an axis passing through that centre, to find with what velocity any great circle on the surface, but oblique to that axis, moves along itself. After the determination of this velocity, Mr. W. infers these 2 corollaries.

Corol. 1. Hence it follows, that in whatever manner a globe revolves, its velocity, measured on the same great circle on its surface, must be the same at the same time at every point of the periphery of that circle.—*Corol.* 2. Consequently, however the plane of a great circle varies its motion, the velocity at any instant is at every point of the periphery equal along its own plane.

Prop. 2. Supposing the centre of a sphere to be at rest, while the surface moves round it in any manner whatever; then, if the same invariable point o, considered as the pole of an axis of the sphere, be itself in motion, the angular velocity of the spherical surface about that axis will be unequable, or that of one point in it different from that of another.—*Corollary.* Hence, about whatever axis the angular motion of a sphere is equable, the pole of that axis, and consequently the axis itself, must be at rest at the instant. Different motions may have different correspondent poles, and consequently, when the motion is variable, the place of the pole of equable motion on the surface may vary; but whatever point on the surface corresponds with that pole must at the instant be at rest.

Prop. 3. Let ABC (fig. 6, pl. 8) be an octant of a spherical surface in motion, while the centre is at rest; and let the velocity of the great circle BC in its own plane $= a$, and in a sense from B towards C; that of CA in the sense from C

towards A $= b$, and of AB from A towards B $= c$. If these 3 velocities a, b, and c, be constant, the spherical surface will always revolve uniformly about the same axis of the sphere at rest in absolute space.

PROP. 4. If a spherical surface, whose centre is at rest, revolve in any manner whatever, so that the velocities along the 3 quadrants bounding any octant of it be expressed by any 3 variable quantities x, y, and z; to find the necessarily corresponding accelerating forces with which the place of the natural or momentary axis, and the angular velocity of the surface round it, are varied.—After the investigation, Mr. W. adds: the preceding general properties of motion obtain in all bodies revolving round a centre at rest, be their motion ever so irregular; the 3 great circles bounding an octant of the spherical surface revolving with the body are also taken ad libitum, being any such circles whatever on the surface; and hence the following very important consequence is drawn, viz. If any body be in motion, or put in motion, by instantaneous impulse or otherwise, about its centre of gravity at rest in absolute space; if, by any means, the accelerating forces acting along the 3 great circles bounding any octant of a spherical surface that has the same centre of gravity and revolves with the body, can be found, those acting at every other point of such surface will necessarily follow as natural consequences of these 3, and thus all the motions of such body will be absolutely determined.

PROP. 5. The same being given, as in the last proposition, it is proposed to illustrate the manner in which the surface moves with respect to a point at rest in absolute space.

PROP. 6. If a parallelopipedon, or other solid, revolving uniformly with an angular velocity $= z$ about one of its permanent axes of rotation, receive an instantaneous impulse in a direction parallel to that axis, the centre of gravity of the body being supposed to be kept at rest by an equal and contrary impulse given to it, and no other force acting on the body; it is proposed to determine the alteration in its motion, in consequence of such instantaneous impulse.

PROP. 7. If a body of any form revolve in any manner whatever with its centre of gravity at rest in absolute space, and so as not to be disturbed by the action of any external force; to determine in what manner it will continue its motion for ever.

We have omitted the analytical solutions of these propositions, both because they are not every where exempt from errors, and because various other solutions of the general problem are to be found as well in these Transactions as elsewhere.

XXV. On the Chronology of the Hindoos. By Wm. Marsden, Esq., F. R. S., and A. S. p. 560.

Unfortunately for the gratification of rational curiosity, history seems to have

been, of all branches of study, that which the Hindoos cultivated with the least care, and we regret to find the periods marked by the revolutions of the heavenly bodies, of which other nations have availed themselves to ascertain and record the important events in human affairs, by them unprofitably applied to the dreams of their mythology. The unremitted labour of ages has been devoted to perfecting the calculation of the lunar motions, in which their correctness is surpassed only by the European improvements of very modern times; but, by a strange perversion, the accuracy thence acquired in their prediction of eclipses, appears to have no other object than that of administering to an idle superstition, which it ought to destroy. Though the fabulous exceedingly prevails in all the ancient documents hitherto introduced to our knowledge, and in none more conspicuously than those which contain the genealogies and reigns of their early kings, yet we are not hastily to conclude, that this people are destitute of records of true history. The portion of their literary stores which we have had opportunity to examine is comparatively small. Perseverance may discover annals, more or less ancient, whose present obscurity is perhaps occasioned by that very circumstance which constitutes their real value—the want of the miraculous. Some authentic monuments have already been elucidated by the learned skill of a gentleman, now a member of the R. S. Facts will accumulate by degrees, and acquire authority by mutually bearing on each other; and the Hindoos, like many other nations of the world, may hereafter be indebted to strangers, more enlightened by philosophy than themselves, for a rational history of their own country.

In different parts of India, and even in one and the same part, we observe various chronological eras referred to, as well in their astronomical treatises, as in their political and private writings. These being productive of confusion, if not clearly understood and discriminated, it is intended here to exhibit such a comparative statement of their respective commencements and coincidences as may tend to remove this impediment to the progress of historical knowledge. The present purpose does not lead to attempt a discussion of what may be termed the factitious periods of Hindoo computation, or to explain the nature and duration of the 4 ages, or Yoogs, which this speculative people, in the wanton exercise of numerical power, have portioned out from the boundless region of time. The 3 former of these divisions, even though the progressive numbers assigned to them should be admitted as the result of astronomical combination, can be presumed to have but little reference to practical chronology, which seems to trace its origin no higher than the commencement of the 4th, or present age, denominated the Kalee Yoog. This constitutes the principal era here to be attended to, and comprehends within it these that follow; the era

of Bikramajit, the era of Salabân, the Bengal era, not strictly Hindoo, and the cycle of 60 years.

Before proceeding to a comparison of their several dates, it will be proper to define the nature of the year and its constituent parts, according to which these eras are computed by the Brahmans, who are the depositaries of science as well as of religion. Their astronomical year is the measure of that portion of time which is employed in a revolution of the sun, from the moment of his departure from a certain star in their zodiac, as seen from the earth, till his return to the same. It is therefore solar and sidereal, and contains, by their calculation, 365^d 6^h 12^m 30^s; and as they suppose the annual movement of the stars in longitude, or the precession of the equinoxes, to be 54″, or 21^m 36^s of time (admitting with them, the sun to move at the rate of a degree each day), their tropical year will be 365^d 5^h 50^m 54^s: but as the sun really moves over 54″ in 21^m 55^s, its length is strictly 365^d 5^h 50^m 35^s, or 1^m 52^s greater than the tropical year as determined by Mayer at 365^d 5^h 48^m 43^s. The true precession being 50″.3, which space the sun describes in 20^m 25^s, the true sydereal year is 365^d 6^h 9^m 8^s, and consequently the Hindoo year exceeds it by 3^m 22^s, or 1 day in 430 years. If the opinion of astronomers is well founded, that a sensible diminution in the length of the year, as well as in the angle of obliquity of the ecliptic, has gradually taken place in the lapse of many ages, it will follow, that this error may not have existed, or been so great, at the period of adjusting the Hindoo tables; and when we consider that there appears no ground to believe their apparatus for observing was ever much superior to what it is discovered among the Brahmans of this day, we are led to wonder at the precision attained to in this determination, and which, in the calculation of the moon's apogee, is still more remarkable. The defect of art can have been compensated only by the remote antiquity in which the series of their observations originated, affording an opportunity of correcting the inaccuracy of particular measurements by a mean of large numbers and distant intervals.

They divide the zodiac into 28 lunar, and into 12 solar constellations or signs, and their astronomical year commences with the sun's arriving at the first point of their constellation of Aries. This division of the zodiac, so far as the accuracy of their observations allows, is connected with the actual phenomena of the heavens, and advances with the apparent motion of the stars, from east to west, leaving gradually behind it the equinoctial points, and is not, like our zodiac, an abstract division of space, attached to those points, and independent of the starry system. Calculating on their principles, the difference of the 2 zodiacs, or the accumulated amount of the annual precession, since the coincidence supposed to be in the year of the Christian era 499, is in the present year 19° 21′ 54″.

The length of their months is determined by the time which the sun employs

in passing each sign, and they are accordingly longer in the apogee, and shorter in the perigee; that which corresponds with the higher apsis being $31^{d}\ 14^{h}\ 39^{m}$, and that with the lower $29^{d}\ 8^{h}\ 21^{m}$ only. It does not appear that the Hindoos are accustomed to enumerate, for civil purposes, the days of the solar month, but to date from the age of the moon that happens to fall within that month, or frequently from the simple phase of the moon.

Their festivals and fasts, like those of the Jews and Christians, being regulated, for the most part, by the lunar revolutions, they employ on this account, exclusively of the solar astronomical reckoning, a lunar year. This they make to consist of 12 months, and each semi-lunation is distinguished into 15 equal portions, or lunar days, which are somewhat shorter than the natural day. In order to preserve its general correspondence with the solar year, "they reckon twice that lunation during which the sun does not enter on any new sign," or, in other words, which falls completely within a solar month; and the obvious reason for this mode of intercalating is, that as the lunar months take their denomination from the solar month in which the change happens, if 2 new moons fall within the same month, they naturally take the same name, and no irregularity is observable. This opportunity of increasing the number of lunar months, without embarrassing the reckoning, presents itself, on a medium of a few years, just as often as is requisite to effect the compensation. It cannot happen in all the solar months indifferently, their lengths being unequal, and some of them shorter than the synodical lunar month.

The commencement of the solar day is usually estimated from sunrise, and the space between that and the sunrise of the following day is divided into 60 parts, the length of which must vary with the sun's unequal course through the ecliptic; but for the purposes of calculation it is supposed to be ascertained at the solstices, and is equal to 24 of our minutes. The subdivisions, in like manner, follow the sexagesimal scale. There is also a mechanical division of the day and night into 8 parts, of which 4 are allowed to the interval from sunrise to sunset, and 4 to that from sunset to sunrise. The proportion of length of these parts respectively depends therefore on the season of the year and the latitude of the place, and the division is consequently inapplicable to general astronomy.

The days of the week are denominated from the 7 planets, and their arrangement is the same with that adopted in the western parts of the world, proceeding from the sun and moon to Mars, Mercury, Jupiter, Venus, and Saturn. Friday, or the day of Venus, appears as the first of the week in their calculations, and probably because the Kalee Yoog began on that day; but in common the week is considered by the Hindoos as beginning with Sunday. Having thus briefly touched on such points of their astronomy as are immediately connected with the

measurement of time, Mr. M. proceeds to the comparison of their eras, which he first brings into one general view, and afterwards considers separately.

Table exhibiting the correspondence of the several Hindoo eras with each other, and with the Julian period and Christian era.

	Julian period.	Kalee Yoog.	Era of Bikramajit.	Christian era.	Era of Salabân.	Bengal era.	Cycle of 60 years.
Kalee Yoog, or grand era..	1612	0					13
Era of Bikramajit........	4657	3045	0				58
Christian era............	4713	3101	56	0			54
Era of Salabân..........	4791	3179	134	78	0		11
Present cycle of 60 years..	6460	4848	1803	1747	1669	1154	1
Year 1790 of J. C. (from April)............	6503	4891	1846	1790	1712	1197	44

For better understanding the above table, it is necessary to remark, that when the Hindoos quote the year of an era, they do it by the number of the elapsed or complete year; whereas, the common European mode is to date by the number of the current or incomplete year; therefore what we should term the first year of an era, is with them the year zero, and their year 1 that which follows; excepting in the cycle of 60, of which the year 1 immediately succeeds the last complete year of the former cycle. The difference in years between any 2 eras is expressed by the number appearing at the intersection of the horizontal and perpendicular lines, to which the names of such eras are prefixed; or it may be found by subtracting the less number from the greater, as they stand in any of the horizontal lines, under their respective names at the top of the perpendicular columns; thus, the years intervening between the eras of Salabân, and that of the Kalee Yoog, are denoted by the number 3179, at the intersection of the 2 lines, or equally by deducting 1712 from 4891, in the lowest horizontal line.

In a comparison of the dates of the earlier Hindoo eras with the Christian era, there occurs a difficulty which it is proper to consider apart. This arises from an ambiguity in our manner of reckoning the years before Christ. It is most usual to pass immediately from the year 1 after to the year 1 before Christ, making the interval of time only 1 year; but some of the best chronologists pass from the year 1 after to the year zero, and thence to the year 1 before; by which means the interval between any number of years before and after Christ is equal to the sum of those numbers; and as this method is used in almost all astronomical tables, it may, without impropriety, be called the astronomical, and the other the common method. As the Hindoo year begins in our month of April, we must observe, in reducing any Hindoo date to the Christian era, that when it happens between the com-

mencement of our year and theirs, the number of their year must be increased by 1, and the subtraction or addition then made.

The Kalee Yoog, or principal chronological era, began in the year before specified, when the sun's mean place was in the first point of the constellation Aries of the Hindoo zodiac; which happened on the 18th of February, at sunrise, under their first meridian, called the meridian of Lanka. At that period, it is said to be asserted by their astronomers, that the sun, moon, and all the planets, were in conjunction, according to their mean places. The reality of this fact, but with considerable modification, has received a respectable sanction from the writings of an ingenious and celebrated member of the French Academy of Sciences, who concludes, that the actual observation of this rare phenomenon, by the Hindoos of that day, was the occasion of its establishment as an astronomical epoch. Though M. Bailly has supported this opinion with his usual powers of reasoning, and though abundant circumstances tend to prove their early skill in this science, and some parts of the mathematics connected with it, yet we are constrained to question the verity or possibility of the observation, and to conclude rather that the supposed conjunction was, at a later period, sought for as an epoch, and calculated retrospectively. That it was widely miscalculated too, is sufficiently evident from the computation which M. Bailly himself has given of the longitudes of the planets at that time, when there was a difference of no less than 73° between the places of Mercury and Venus. But 15 days after, when the sun and moon were in opposition, and the planets far enough from the sun to be visible, he computes that all, excepting Venus, were comprehended within a space of 17°; and on this he grounds his supposition of an actual observation.

Calculating from the rules laid down by the Brahmans, as given by M. Le Gentil, it appears, that 4891 mean years of this era expired on Monday, 12th April, 1790, at $4\frac{1}{4}^h$, and that the true place of the sun came to the first of Aries, and consequently that the 4892d year, or 4891 complete as the Hindoos express it, actually began on the 10th, at 1^h; or on Sunday, 11th April, in their civil way of reckoning. Thus we see that, during this long period of time, the Hindoo account has lost on the Julian 42 days, allowing for the change that took place in our stile. The year of the former exceeding the latter by 12^m 30^s, falls continually later and later on our old stile year, at the rate of a day in about 115 years; and from this the commencement of any future year may be readily computed. The annual irregularity observable, which is independent of the almost imperceptible change, arises only from our mode of intercalating a day at the end of every 4th year, to compensate the fractions that have accumulated during that time. The Hindoo astronomical year, admitting of no intercalation, cannot preserve an annual correspondence, but began at nearly the same time, with

respect to our year, in 1786 as in 1790; in 1775 and 1787, 6 hours later; in 1781 and 1785, 6 hours sooner; and in 1784 and 1788, being leap years, 18 hours sooner than in 1787. The civil year begins at the sunrise immediately preceding the calculated commencement of the astronomical, when that happens during the day; but if in the night-time, it begins at the sunrise following, and will consist of 366 days as often as the excess of the astronomical year above 365 amounts to a whole day.

The next era that presents itself and the first that has pretensions to be considered as historical, is that of Bikramajit. This prince, whose paternal kingdom was Malva, and his capital Ougein, waged war against Saka king of Dehli, probably his lord paramount, and having overcome and slain him, ascended in his stead the principal throne of India. Authorities differ widely as to the length of his reign; but he likewise is said to have fallen in battle, fighting with a king who invaded him from the southern provinces. He appears, from this account, in no other light than that of an unfortunate usurper, yet his fame in the memory of the Hindoos has eclipsed that of his predecessors, his adventures are a favourite subject of romance, and an era has been distinguished by his name. It is doubted whether we ought to consider his accession, or his death, as constituting the proper epoch; but with regard to the time from which the reckoning dates there is no uncertainty, and the nature of the event is not essentially connected with our present object. It is placed in the year 56 before Christ, or in the 4657th of the Julian period, and corresponds with the 3045th year of the grand era.

Mr. M. remarked, when treating of the Hejerà, that this Mahometan era was computed, not from the day of the prophet's flight, as generally supposed, but from the ordinary commencement of that year in which the flight happened; and thus we find, on comparing the Hindoo eras, that though some of them are professed to be counted from the deaths of their kings or other historical events, they yet all begin from the same point of the sun's annual course through the zodiac. The numerical reckoning of the years can well be conceived more liable to arbitrary change, as being less interesting to the bulk of the people, than the observance of a particular day, whose periodical return is every where marked with popular ceremonies and superstitions.

The era to which Salabân has given name dates from the 78th year of Christ, or 4791st of the Julian period, commencing with the 3179th year of the grand era. As this is no less than 134 years later than that of Bikramajit, it seems a bold anachronism to make them cotemporaries, or to suppose, what is commonly asserted, that the one prince was the conqueror of the other. Fortunately for the present investigation, it is history rather than chronology which suffers from this want of accuracy or discrimination in the annalists of India, or their Persian

translators. Salabân, who is said to have reigned many years over the ancient kingdom of Narsinga, in the northern part of the peninsula, is described as a liberal encourager of the sciences. There is reason to think, that astronomy experienced a reformation and considerable improvements under his auspices; and the professors appear to have attached the celebrity of an era to his death, in respect for his talents and gratitude for his protection.

As the era of Bikramajit prevails chiefly in the higher or northern provinces of India, so does that of Salabân in the southern, but more exclusively. In their current transactions, however, the inhabitants of the peninsula employ a mode of computation of a different nature, which, though not unknown in other parts of the world, is confined to these people among the Hindoos. This is a cycle, or revolving period, of 6 solar years, which has no further correspondence with the eras above-mentioned than that of their years respectively commencing on the same day. Those that constitute the cycle, instead of being numerically counted, are distinguished from each other by appropriate names, which, in their epistles, bills, and the like, are inserted as dates, with the months, and perhaps the age of the moon annexed; but, in their writings of importance and record, the year of Salabân, often called the Saka year, is super-added; and this is the more essential, as it is not customary to number the cycles by any progressive reckoning. In their astronomical calculations, they sometimes compute the year of their era by multiplying the number of cycles elapsed, and adding the complement of the cycle in which it commenced, as well as the years of the current cycle; but from hence we are led to no satisfactory conclusion respecting the origin of this popular mode of estimating time. The presumption is in favour of its being more ancient than their historical epochs. The present cycle, of which 43 complete years were expired in April 1790, began in the year 1747, with the year of Salabân 1669, and of the grand era 4848. M. Le Gentil, to whom Europe is chiefly indebted for what is known of Hindoo astronomy, has fallen into an unaccountable error with regard to the years of this cycle, and their correspondence with those of the Kalee Yoog, as appears by the comparative table he has given of them, and other passages of his work. He seems to have taken it for granted, without due examination, that the year 3600 of the latter must have been produced by the multiplication of the cycle of 60 into itself, and consequently that the first year of this grand era must also have been the first of the cycle; but this is totally inconsistent with the fact; the Kalee Yoog began with the 13th year of the cycle of 60, and all the reasoning founded on the self-production and harmony of these periods must fall to the ground.

It now remains to take notice of a mode of reckoning peculiar to the province of Bengal, and thence denominated the Bengal era. The circumstances of its

institution are involved in obscurity, and I do not find that even a conjecture on the subject has yet been offered to the world. It is admitted however to have been imposed by the Mahometan conquerors, and is therefore of no very remote antiquity. The most obvious consideration that presents itself, in examining the date of this era, is its proximity to the year of the Hejerà; the Bengal year 1196 complete ending on the 10th April, 1790, and the Hejerà year 1204 on the 10th September following. The difference has plainly arisen from the inequality of the solar and lunar reckonings, and its accumulation since a certain period when they must of necessity have coincided; and it is no improbable supposition, that the time of such coincidence was also that of introducing the mode of computation which has since prevailed. By ascertaining the amount of this difference, and the number of years required to produce it, we may expect to arrive at a knowledge of the period in question, or to approximate it at least. As the Hindoos compute from the elapsed year, and the Mahometans by the current, the difference between the two dates should be 6 years and 7 months; but as this correction of 1 year may be presumed equally applicable at the supposed time of the coincidence, and therefore unnecessary in this instance, it will follow that the real difference should be found by a simple subtraction of one date from the other, and consequently be 7 years and 7 months. The annual excess of the Hindoo or sydereal year above the Mahometan or lunar being $10^d\ 21\frac{1}{4}^h$, an interval of 254 years is required to produce this difference, or 220 years, to produce that of 6 years and 7 months. The former number being deducted from 1790, carries us back to the year 1536, in the reign of the Mogul emperor Humaioon, and the latter to 1570, in that of Akbar. About one or other of these periods we should seek for some record of the institution, but the histories we possess throw no light on the subject. Several gentlemen, conversant in the affairs of Bengal, to whom I have referred it, confirm the general idea as above given, but disagree as to the political circumstances. By one, the regulation is ascribed to Sheer Shah, who wrested the empire from Humaioon, and governed it for some years with wisdom and energy, when his death, and the distractions that ensued, restored it to the former possessor; and, by another authority, to Akbar, deservedly named the Great, who was next in succession.

The most obvious way of accounting for the peculiar mixture of Hindoo and Mahometan observances in this reckoning, appears to be, that the zeal of the monarch for establishing the era of his prophet had effected only a partial or temporary innovation; and that his new subjects, who were constrained to adopt as an epoch that year of the Hejerà in which the royal edict bore date, could not ultimately be forced to change their accustomed solar year for one that rapidly inverted the order of the seasons. But this attempt must be referred to a period somewhat earlier than the reigns of these particular princes, which were dis-

tinguished by a liberal policy; and the gentlemen above alluded to attribute to them respectively, not the interdiction but the restitution of the solar reckoning; that of the Mahometans, imposed on the province by their more bigotted predecessors, being found inconsistent with the periodical collection of the revenue, which depends on the harvests. It is, in truth, extremely difficult to conceive how a mode of computation, so much at variance with the rational concerns of civilized people, can possibly subsist in any state of society above that of the pastoral and predatory tribes with whom it originated.

As it appears, that the people of Siam, in the farther India, have borrowed their knowledge of astronomy from the Hindoos, it will not be thought inconsistent with the present subject, to add some account of the chronological eras in use among them. Of these, one has been termed their civil, and another their astronomical era. The civil reckoning is by lunar years, consisting of 12 months each, with an intercalation of 7 months in the period of 19 years, and commencing with the new moon that precedes the winter solstice. This era is computed from the supposed time of the introduction of their religion by Sommona-codom, 544 years before Christ, or in the year of the Julian period 4169; and consequently 2333 years of it were expired in the month of December 1789; but by a custom which, though not without its parallel, wants to be satisfactorily explained, they do not change the date, or count the succeeding year 2334, till it meets the astronomical reckoning in the month of April following.

The astronomical era is founded immediately on the tables and modes of calculation adopted from the Hindoos. The French astronomer, Dom. Cassini, by an ingenious deduction from no very circumstantial data, inferred that it must have had for its epoch a mean conjunction of the sun and moon, which happened on March 21, 638 of the Christian era. This preceded by a few hours the commencement of the established Hindoo year, to which it was evidently meant to be accommodated, though it is by him referred to the vernal equinox, which took place 2 days earlier. The length of the Siamese solar year he found to be 365^d 6^h 12^m 36^s, and consequently 1152 years of the era should expire on the 11th April 1790, when the sun enters the Indian zodiac, being 560 years later than the era of Salabân. For want of corroborating facts, this determination of an epoch by M. Cassini was considered as speculative and uncertain; but Mr. M. was in possession of a date which, though not precise, may serve generally to authenticate it. "In 1769, the king of Pegu (a country bordering on Siam, and formerly conquered by it) dates his letter to the French at Pondicherry, the 12th of the month Kchong 1132." This makes 1790 to correspond with 1153 instead of 1152; but when we consider the vague manner in which notices of this kind are given, a difference of 1 year can scarcely be urged as an objection.

The Siamese were also accustomed to make use of a cycle of 60 years, ex-

pressed by a repetition of 12 names of certain animals, which are for the most part the same with those employed, for the same purpose, by the Chinese and Mogul Tartars, from whom we may conclude it has been borrowed; but the meagre and unsatisfactory examples of its application, furnished by M. Loubere and P. Tachard, do not afford us the means of determining at what time they began to reckon their cycle. It appears only that the year 1687 was the 10th of the lesser or constituent cycle of 12 years.

END OF THE EIGHTIETH VOLUME OF THE ORIGINAL.

END OF VOLUME SIXTEENTH.

C. and R. Baldwin, Printers,
New Bridge-Street, London.

Fig. 1. Pa. 6.

Fig. 2. Pa. 6.

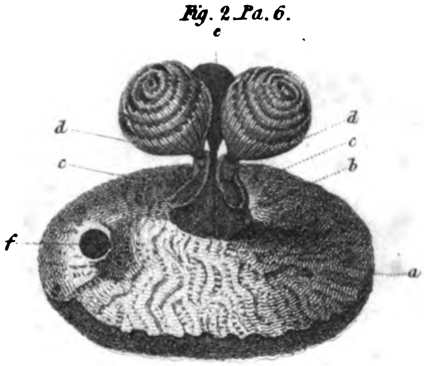


Fig. 3. Pa. 8.

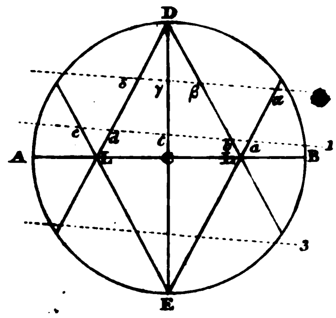


Fig. 4. Pa. 15.

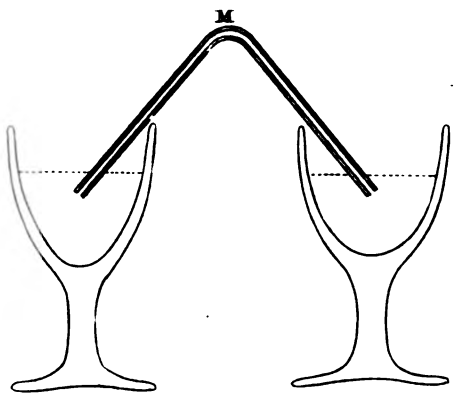


Fig. 5. Pa. 16.

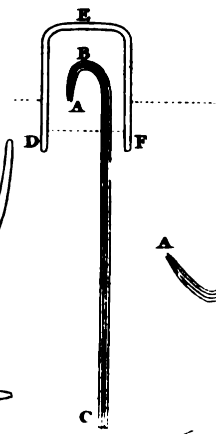


Fig. 6. Pa. 16.

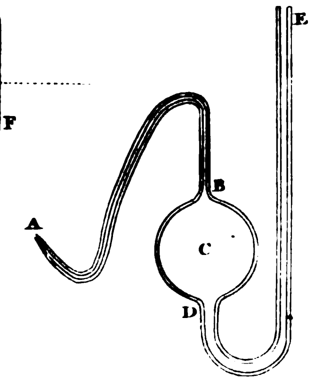


Fig. 7. Pa. 136.

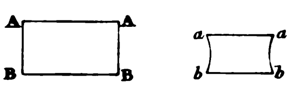


Fig. 8.

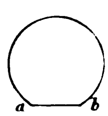
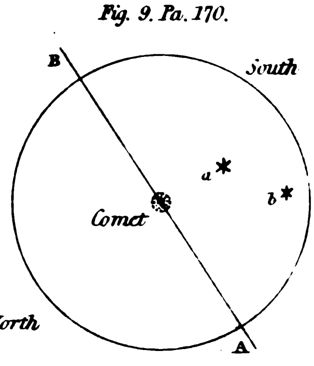
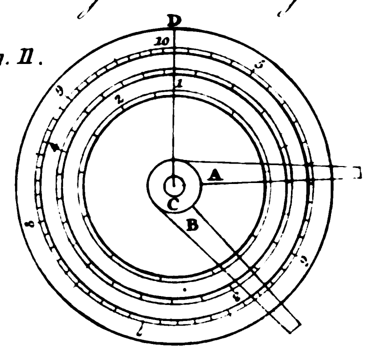


Fig. 9. Pa. 170.



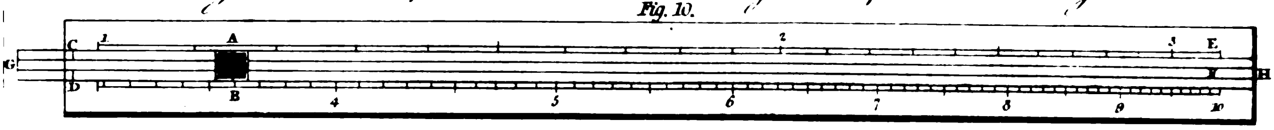
Instrument equivalent to the Gunter's Scale in Fig. 10.

Fig. 11.



Gunter's Scale, equivalent to those commonly made of 28½ Inches long.

Fig. 10.



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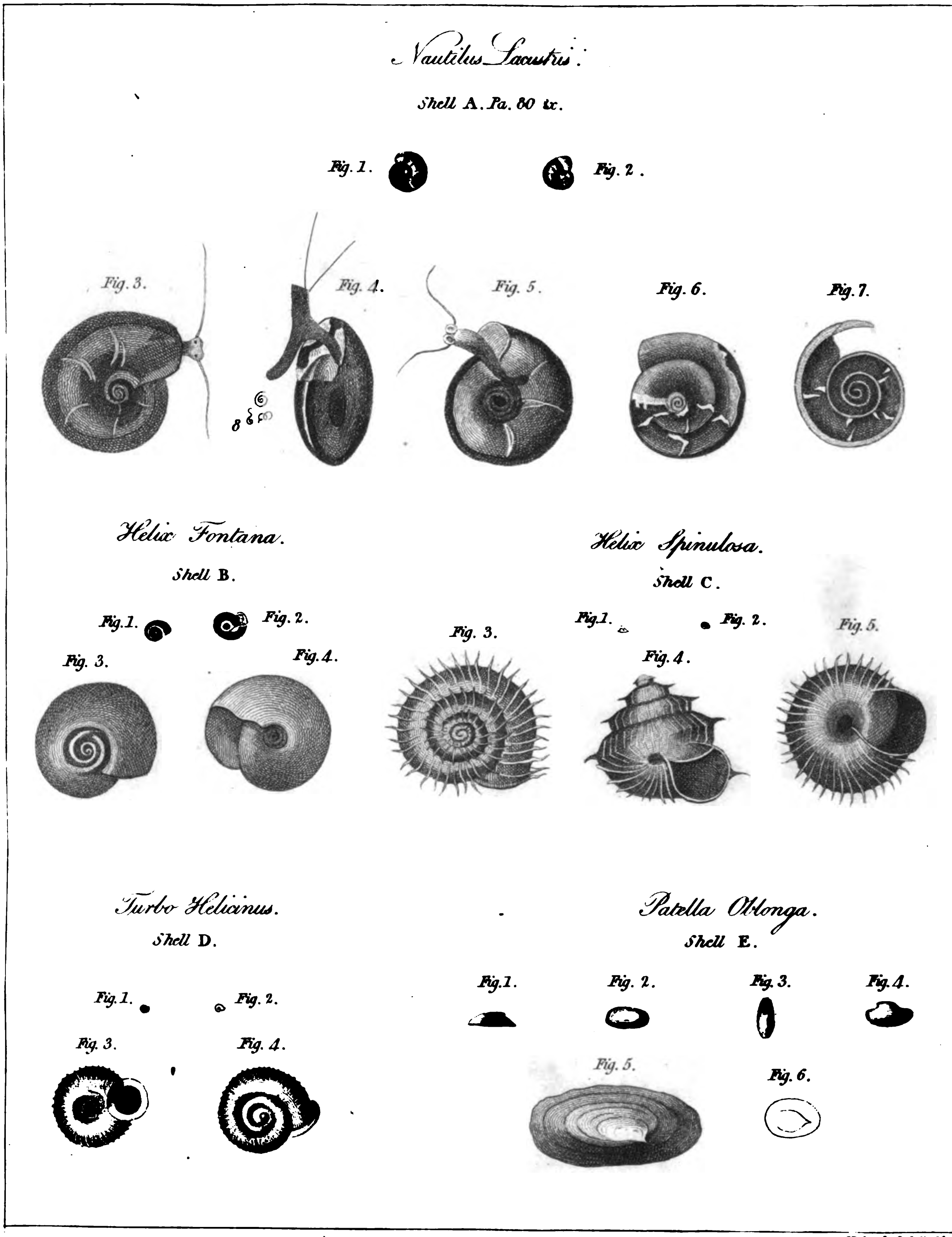

Mutlow Sc. Russell Ct.

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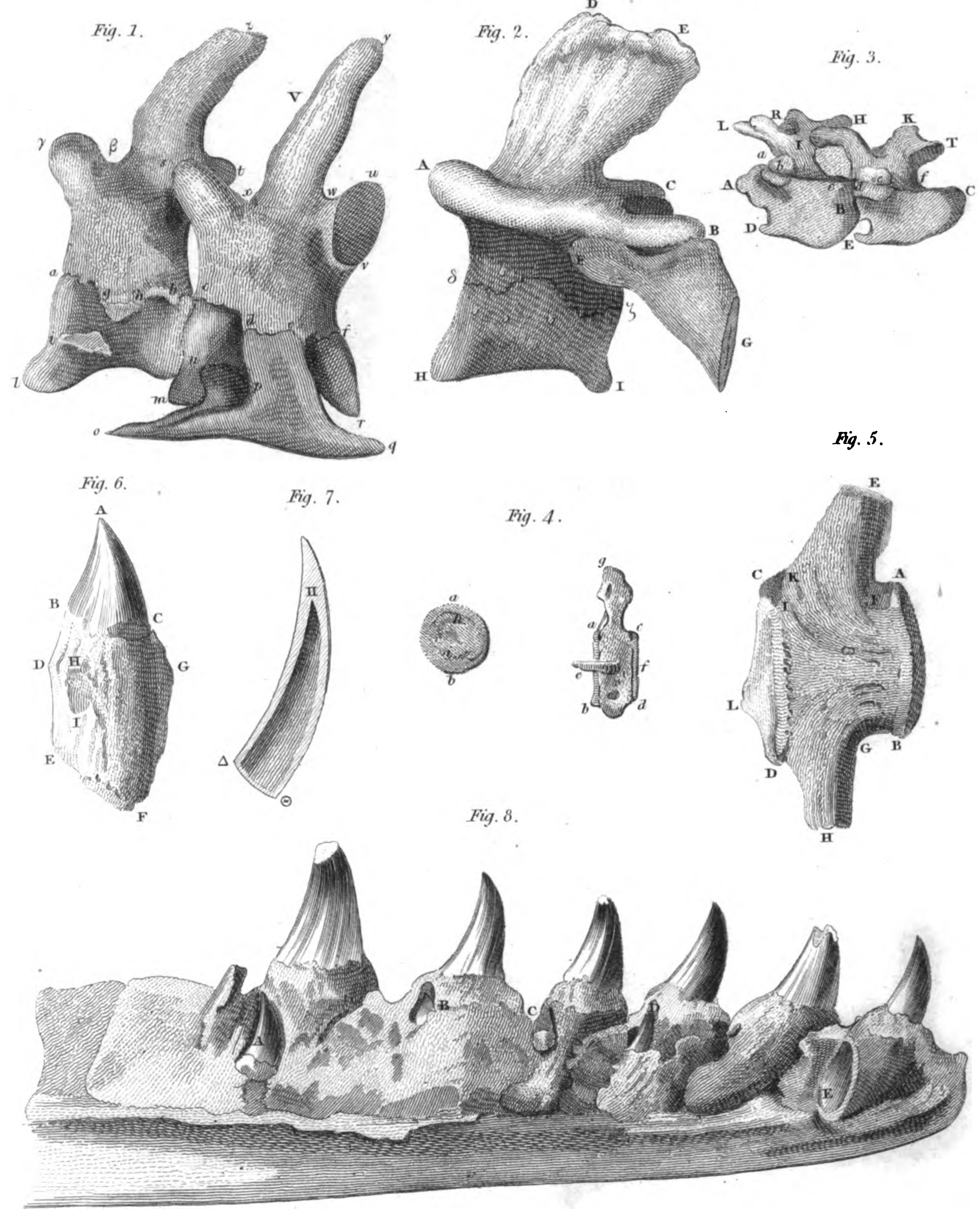
Fossil Bones, reduced from the original, pa. 153 &c.



Mutlow Sc. Russell Ct.

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Fig. 1. pa. 303.

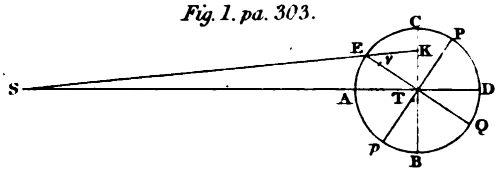


Fig. 2. p. 304.

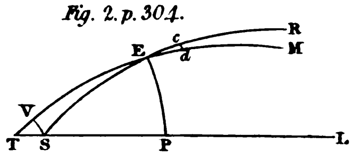


Fig. 4.

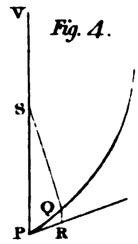
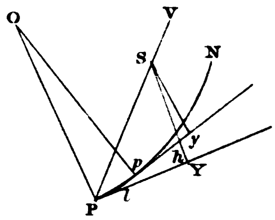


Fig. 3. p. 304. &c.



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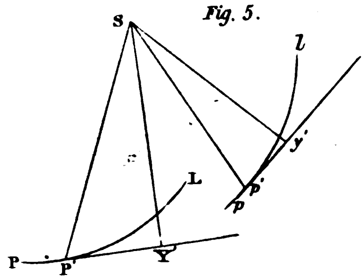

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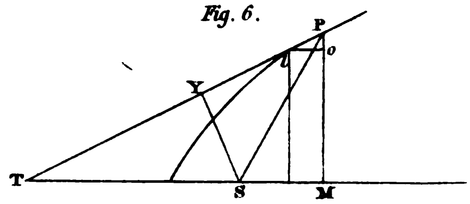

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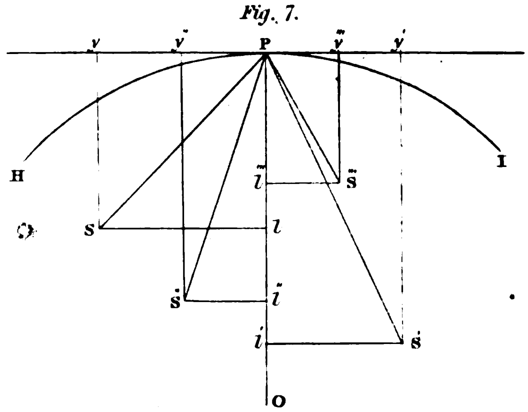

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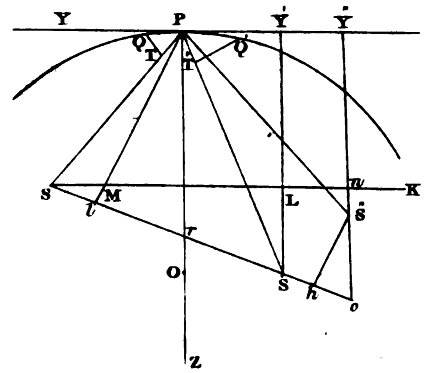

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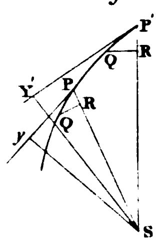

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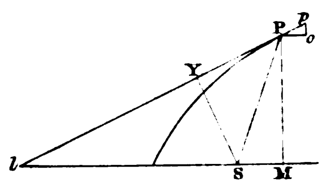

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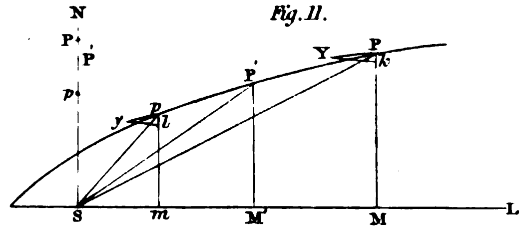

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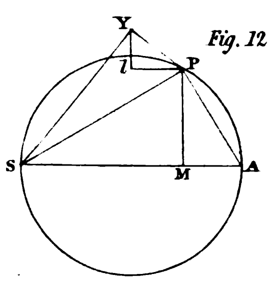

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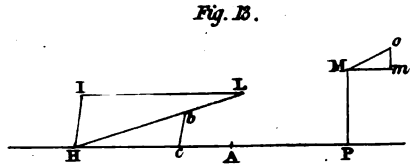

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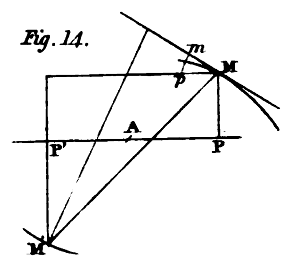

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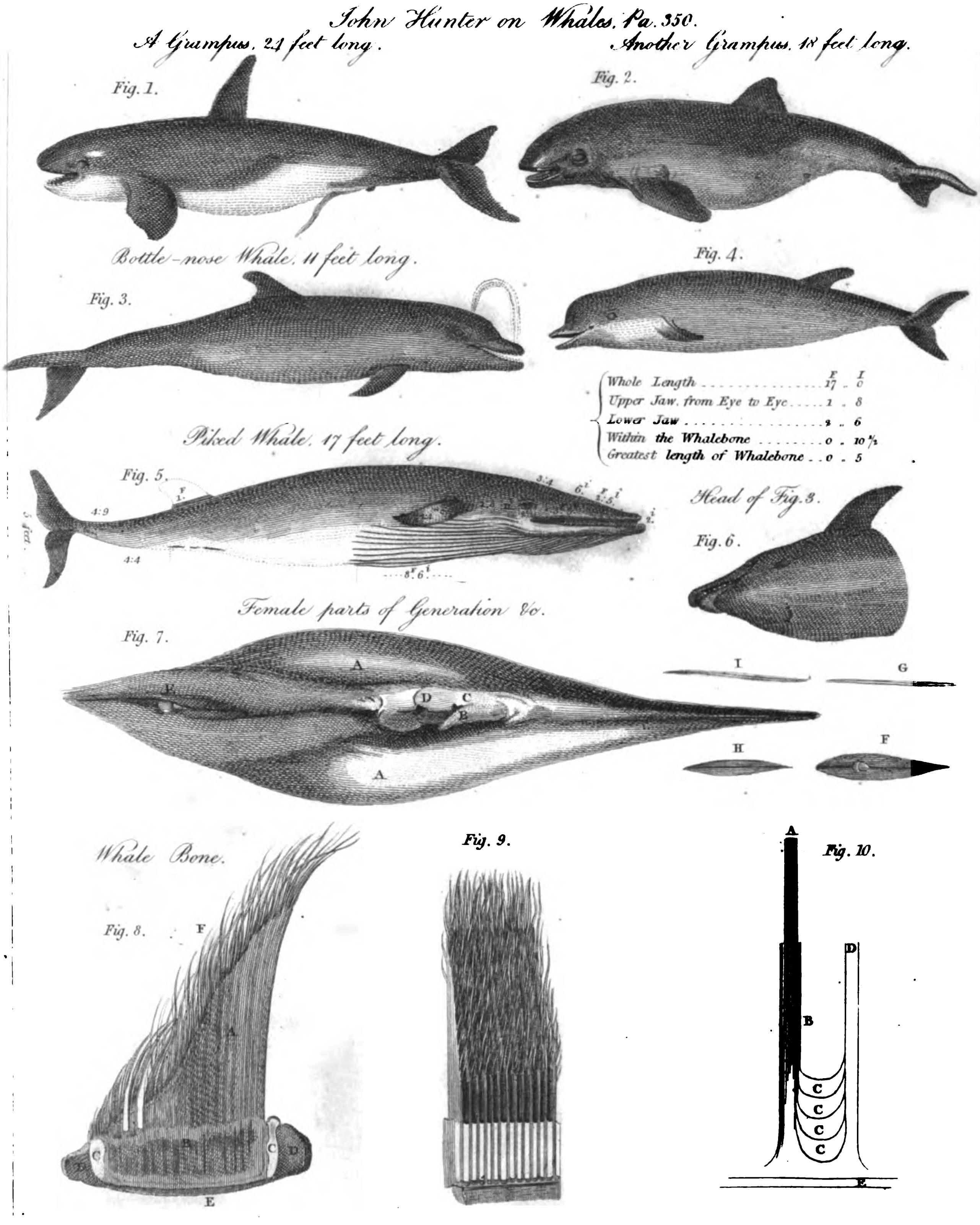

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Fig. 1. Pa. 450.

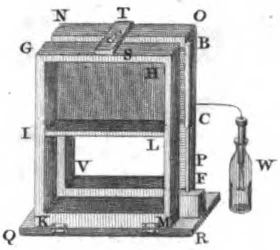


Fig. 2. p. 450.

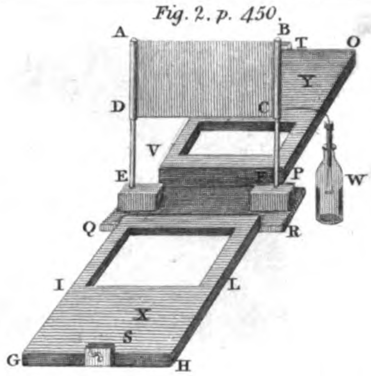


Fig. 3. p. 505.

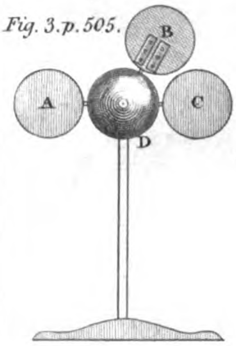


Fig. 4. p. 505.

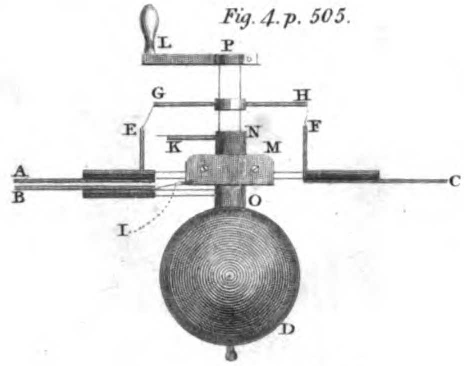
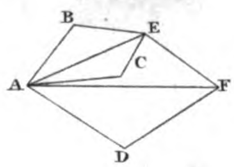


Fig. 5. p. 572.



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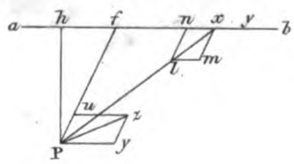

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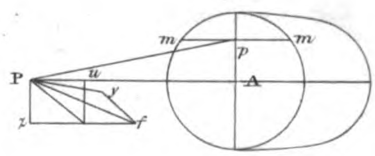


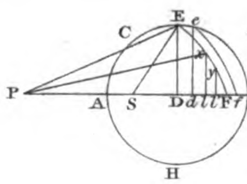
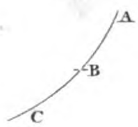


Fig. 8.

Fig. 9.



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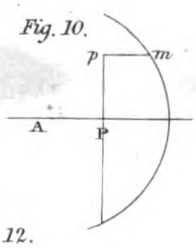

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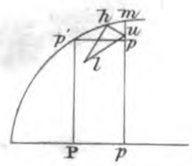
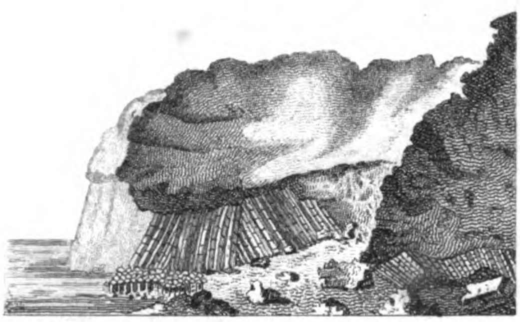


Fig. 14. p. 641.



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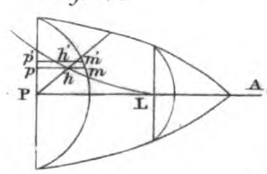

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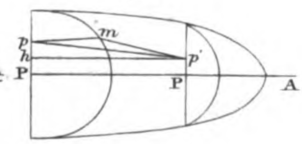
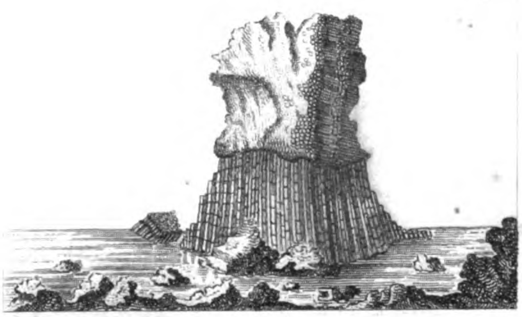


Fig. 16. p. 643.

Fig. 15. p. 641.



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Dr. Herschel on Saturn's Ring and Satellites. P. 613. &c.

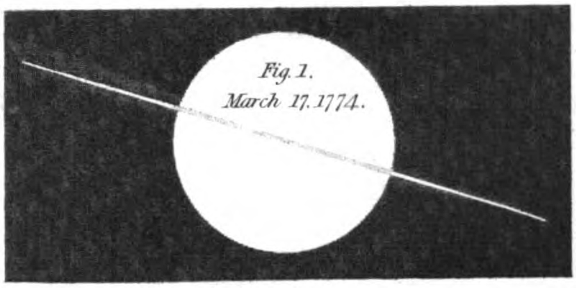


Fig. 1. March 17. 1774.

Fig. 2. April 3. 1774.

Fig. 3. June 20. 1778.

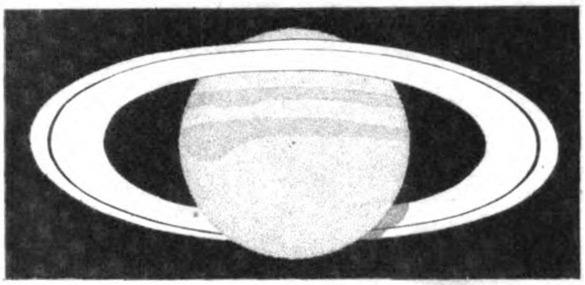
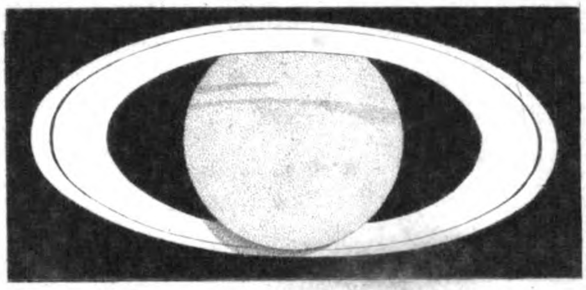


Fig. 4. May 11. 1780.



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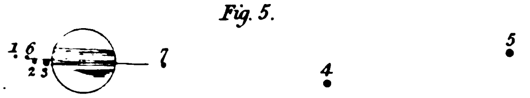

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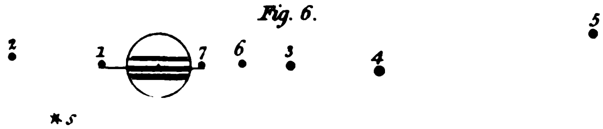

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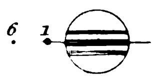

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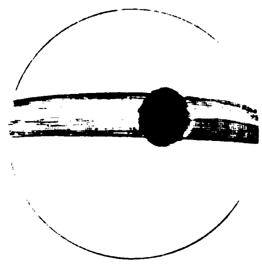

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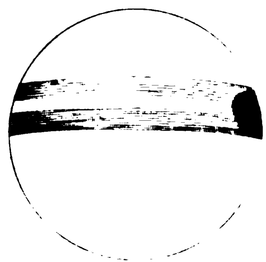

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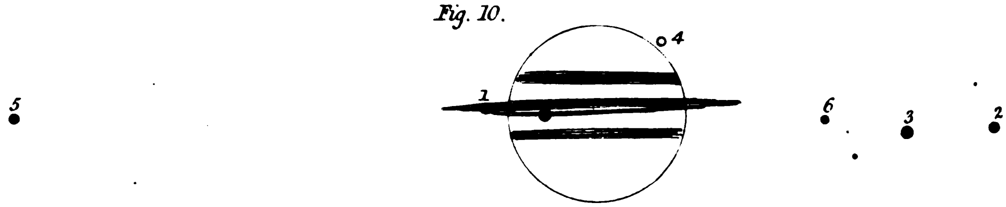

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Spikenard.

Fig. 1. Pa. 662.

Fig. 3. p. 667.

Fig. 4. p. 667.

Fig. 2. p. 667.

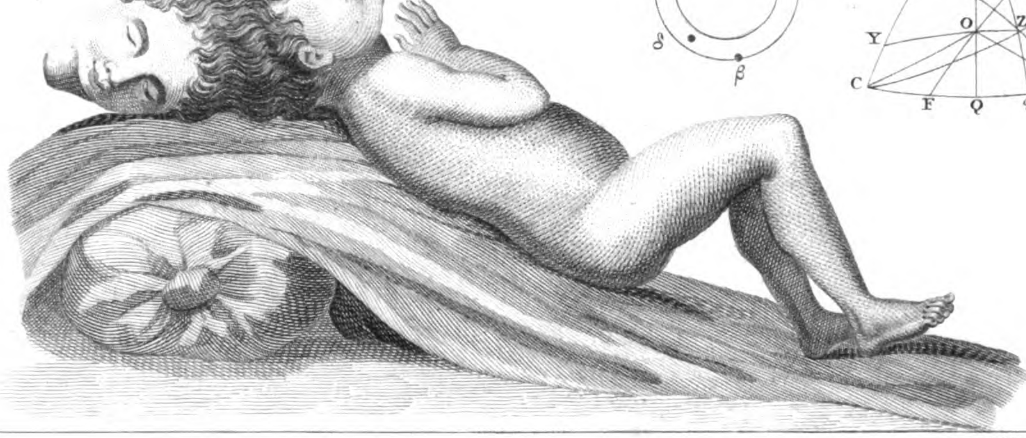


Fig. 5. p. 735.

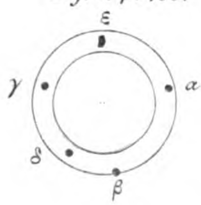
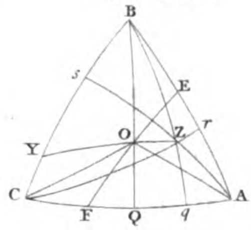


Fig. 6. p. 741.



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